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Original Articles.

DIAGNOSIS OF OVARIAN TUMOR.

Extracts from a Clinical Lecture delivered by EDW. BORCK, A.M., M.D., Professor of Surgery in the College for Medical Practitioners, St. Louis Mo.

GENTLEMEN; In our last lecture we considered abdominal tumors in general, and studied the various modes by which we are enabled to make a correct diagnosis, and under that head included also herniæs. To-day we will take up a class of tumors to which that fair sex, whom we all adore and love so much, alone is heir to: Ovarian Tumors.

And as I speak to men that have had years of experience in their profession and love their study, I take it for granted that you are all acquainted with the pathology—I need not occupy your time with repeating what you can read; as usual you find a roster upon the wall, which will inform you of the papers and articles lately written upon that subject. Let us consider the diagnosis and take a simple case of ovarian tumor for our guide, for it matters not what kind of an ovarian tumor we have, so we know it is ovarian and nothing else, and do not mistake it for other tumors or swellings or the reverse.

For convenience sake, the development of an ovarian tumor may be divided into four stages. Now let me make a diagram: On the left side the tumor begins to grow; as long as it is within the pelvis it is in the first stage; if it grows up to the umbilicus, it is in the second stage, from here to epigastrium, it is in the third stage, and up to its highest point. In the fourth stage its prominence and circumference is alone increased. Between those boundry lines you may make subdivisions again if you choose. And the reason why these tumors occur more upon the left side, is owing to the fact that the left ovarian veins have no valves.

Now let us reflect for a moment and see what disturbance we may expect to be produced by such a growth.

In the first stage: If the uterus is in a normal position, the tumor is behind the uterus and in front of the bladder. There is irritation of the bladder, dysmenorrhœa, constipation, a feeling of a heaviness in the pelvis and hemorrhoids, the latter are more frequent with polycysts.

In the second stage: What must it do? It must displace the small intestines to the opposite side, and as it has arisen out of the pelvis the

uterus is therefore placed behind the cyst, and the bladder goes with the uterus. The tumor by this time has acquired a pedicle and is moveable the patient discovers it rolling about as a ball, if no adhesion has been formed. There is a desire to urinate, but diminished action of the kidneys.

In the third stage: The small intestines are pushed up, the large omentum is only between the tumor and the abdominal walls, it is up to the epigastrium, presses upon the stomach and diaphragm, elevates the ribs, interferes with respiration and digestion, produces palpitation of the heart, and the general health fails. We have derangements of menstruation, emaciation of face and neck and upper extremities. There is a peculiar expression of the countenance, enlargement of the abdominal veins and œdema of the lower extremities.

In the fourth stage: The tumor expands in all directions where there is no resistances, all the symptoms are aggravated, the pulse runs up to 120—130°.

I made these preliminary remarks to refresh your memory. Now suppose a patient comes to us with symptoms like this:—her abdomen has increased gradually in volume, without any sickness; she tells us the swelling first appeared in the iliac region and extended upwards; she has also observed in the beginning of the growth, by turning quickly in bed, a feeling like a ball rolling about in her abdomen. If the swollen abdomen appears like an advanced pregnancy, with well and equal fluctuation, if there is a dull sound in the anterior abdominal region in every position, but on the sides a tympanitic sound upon percussion, if by the motions we impart to the swelling the uterus moves along but is of normal depth, then we can suspect an ovarian cyst. The whole character of the swelling speaks in favor of a cyst, the passive movements of the uterus along with the tumor indicates that the tumor belongs to the internal generative organs. The passive movements produced with the uterus and the normal size of the same, indicates to us that the swelling does not belong to the uterus; it must be an ovarian cyst. And we can prove this positively by making examination per rectum—and here I will say to you, make your examinations always for healthy organs, do not hunt for an ovarian cyst, but for two normal ovaries—if you find both in their physiological condition, exclude disease of the ovary, the swelling must be something else. If you find but one ovary normal, and the other mentioned symptoms in addition, you will not go amiss to say ovarian tumor.

Aspiration of the fluid and the chemical and microscopical examination will clear up any doubt. The chemical character is principally albumen, extractive matters, fat and salt united with water, but it contains no fibrin, and the fluid is not spontaneously coagulable (here the microscopical appearance of what the fluid reveals was shown, after Drysdale). Now after this you may think it easy to make a correct diagnosis, but be

not deceived, it is sometimes very difficult. Let me relate to you the history of a case: An American lady 25 years old, married, had two children, one living, 5 years old, she informed me when I first saw her in consultation with the physician then in attendance, that three years previously she noticed a swelling in her left iliac region, which gradually grew larger; she felt that something was rolling about in her abdomen. She kept it secret and at last she became so large, that her friends thought her pregnant with twins or triplets. She had suffered all this time with intermittent fever. Six months previous to my seeing her she had gone North and came home improved in health. Physicians had recommended tapping, which was not resorted to. She had passed a good deal of water at times, could not tell exactly, but believed it came from the bladder, then became more natural in size, still a large lump remained. The last three months she grew worse, the swelling became hard, every sixth day she would have an attack of fever and pain in abdomen. "Inflammatory attack, Peritonitis."

When I first saw this patient, and before knowing anything about her particulars of the history of this case, I ventured to say to the attending physician, that I expected to find an ovarian cyst, simply by the characteristic expression of the face. Upon an examination, I found a large tumor in the left side, extending up to the umbilicus. Near the umbilicus a distinct fluctuation, which I recognized as an abscess, or possibly nature's effort to open in that region; adhesion only on right side and in front of tumor, left lower side none; I could lift up the abdominal muscles there. Fluctuation otherwise not distinct, the whole tumor somewhat moveable, uterus normal, and pushed to right side. Per vagina tumor could be felt; per rectum, right ovary plain, left not detectable, but tumor distinct. Great irritability of the stomach. She had been seen by a host of physicians, some eminent and well known, but no opinion was given; that is no one committed himself to a positive diagnosis. Poultices, liniments, etc., had been applied externally, also internal medication, all to no purpose, only temporary relief obtained; she was very weak, no cancerous cachexia, no family history of cancer. I diagnosed positive an ovarian tumor, but gave a guarded opinion in regard to complications. Recommended a tonic regimen to build her up. She recruited nicely, and when I saw her again ten days latter I was informed that she passed something like fluid and matter, amounting to a quart, from her bowels. She was anxious to have something done to relieve her suffering. I recommended an operation, and carefully stated to all concerned, that I would make an exploratory incision and then if advisable go on with the operation, otherwise abandon it. They all gladly consented in good hopes;—Now what do you expect? After opening the abdomen, I found a whole mass of adhesion, omentum, mesentery, intestines, all adherent together and to the abdominal walls;

the abdominal walls cancerous; the fluctuation we detected near umbilicus turned out to be an encysted abscess; while cutting through the abdomen in the linia alba, a crackling sound could be heard; no where could an opening be found, all one mass of adhesion. My assistants all looked at me, and I tell you I felt like I had made a mistake, or that I had been deceived in my diagnosis. I could not apprehend that I was mistaken and finally I succeeded in getting my finger between the walls of the abdomen, and hunted for the left ouary. I assure you I felt relieved and confess, a little proud, when I exhibited a plain tumor, of that ovary about as large as a French turnip, a shriveled up sac without fluid. I could peel it out of the surrounding cancerous mass.

The patient's fate was sealed, but it was a instructive case—an ovarian cyst, which nature had tried to cure spontaneously, and a development of maglinant growth not from the tumor, but from other parts and from other unknown causes, surrounding, or better, enveloping the tumor. They are perplexing cases! It is not one symptom alone, but all combined should be taken into consideration and carefully studied and weighed.

Pregnancy, encysted dropsy of the peritoneum, uterine fibroid tumors, distended bladder, renal tumors and cysts, cysts of the broad ligament, "these by the way are considered as the most difficult to diagnose" Ascites, etc., etc., have all been, and may again be mistaken for ovarian tumors. Again, ovarian cysts may be complicted with pregnancy, ascites, uterine fibroma and second cyst.

Ascites is mistaken the most frequently for ovarian dropsy, though that mistake ought not be made by any practitioner.

Now in ascites the intestine floats upon the fluid, consequently in the recumbent position of the patient, the dull sound must be heard at the lowest points, the tympanitic sounds upon the highest point, of the abdomen. In ovarian cyst the reverse. If the patient change her position in ascites, the fluid will always gravitate to the lowest point, the intestine floating upon it. In ovarian cyst it remains the same. It is also well to remember that œdema of the extremities appear after tumor is developed, but often precedes in ascites. As I have said to you of other surgical affections, that it is not well nor prudent for the surgeon at once to go to work with his hands and feel and twist and pinch the parts injured, but should first of all inspect with his eyes; and educate them as perfectly as possible to observe the different outlines; always having in his brain the normal contours and position, and then mark the abnormal. Here we can learn a good deal to our advantage with the aid of our eyes, look at these drawings.

We can see at once the difference in the shape and out-lines, it needs no explanation. Observe the difference between the sternum and the umbilicus, and pubis, and look at the umbilicus itself, it may be obliter-

ated in ovarian dropsy, but never presents an arching like it does in ascites, etc. One point I wish to call your attention to in ovarian cyst, you can grasp the abdominal walls between your fingers and lift them up from the tumor, wherever there is no adhesion; in ascites you can not do it.

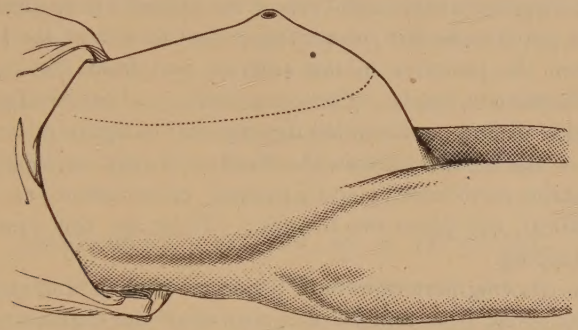

FIG. 1 is a lateral view of the abdomen affected with ascites.

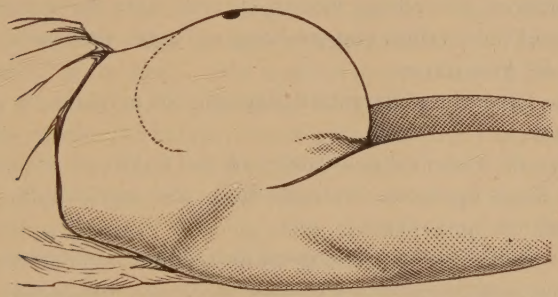

FIG. 2 is a lateral view of the abdomen affected with an ovarian cyst.

The symptoms and signs of pregnancy I need not explain to you, you understand them so well, but let us remember that in a patient affected with an ovarian cyst the breast may become enlarged, and an areola around the nipples may be present, also morning sickness.

Of the diagnosis of uterine fibroid tumors, you will hear all about that from Prof. McIntyre in his course of lectures.

In regard to a distended bladder, all I wish to say is this: never under any circumstances depend upon the nurse or even the patient herself; they may be honest in telling you that she has just passed water, and a good deal of it. I lately met with a case and received such information, yet I introduced the catheter and drew off a quart of fetid urine. Make sure yourself, introduce the catheter every time before examining. Recollect the case on record I related to you of a child where the urethra was closed and the bladder adhered to the umbilicus and at or near that place, was a fistulous opening, wherefrom the urine dribled; she was operated upon at the age of 18 years, and a passage made at the natural

situation; the fistulous opening closed spontaneously. We will meet sometimes with adhesion of the bladder at that point, as a complication with ovarian cyst. Again, there are cases on record where an ovarian cyst became adherent to the umbilicus, and formed a fistulous opening and a spontaneous cure took place.

How to diagnose tumors and cysts of the kidney I have already spoken to you in a previous lecture; don't forget that tumors of the kidneys will enlarge from the posterior to the anterior part from above; and they push the intestine in front. The colon here is always in front and can be filled with air and be distended thereby and recognized.

In cyst of the broad ligament, the fluid is always as clear as spring water, contains no albumen; and a manual examination per rectum under chloroform, will detect two ovaries. These are the cysts that are cured by tapping.

Now this drawing here represents a side view of an abdomen affected with a multilocular cyst, and this is a portrait of the characteristic feature (facies ovariana, after Wells). The time of one hour is too short to go into details about everything, but to do you and the subject justice, I have prepared these tables you see hanging upon the walls, which you may study for yourselves.

Here you have the differential diagnosis of a mono, a poly, and a dermoid cyst.

Then here the chemical consistence of the fluids of an ovarian cyst, cyst of the broad ligament, amniotic fluid, and ascitic fluid. And here what the microscopic reveal.

This all will give you a pretty good idea of the difficulties we may encounter, and what is necessary to make a correct diagnosis; and in conclusion I will say to you again: Be systematic in all your doings; only by following and carrying out a certain system, can we expect to come any ways near to perfection. Let me repeat then.

Take first the symtoms into consideration, the history of the case, the expression of the face and neck, the activity of the kidneys, the sympathetic affections of the mammæ; then the local signs; the rationale as detected by the patient; then the physical local signs; exploration, such as, inspection, menstruation, palpitation, percussion, auscultation, change of position, vaginal touch, trocar, miscroscope; and last of all exploratory incision.

Second: Ask yourselves the following questions, so beautifully illustrated by the late Dr. Peaslee, and answer each and every one of them positively, viz.:

Is there actually an enlargement within the abdominal cavity?

Is there fluctuation, indicating an accumulation of fluid within the abdomen, or a solid tumor? "Mesenteric fibroma or fibro-plastic."

Is the fluctuation due to ascites?

Does the cause of the enlargement arise in the pelvis?

Is not the tumor a pregnant tumor?

Is there not still an enlargement of the uterus though it be not gravid? "Hæmatometra, hydrometra, physometra, uterine hypertrophy, fibroma, carcinoma of fundus, uterine fibro cysts."

And to what is the fluctuation due? Serous cyst of the broad ligament, encysted dropsy of peritoneum, dropsy of fallopian tubes, renal cyst, hepatic cyst, pelvic abscess, splenic abscess, etc."

In our next lecture we will take up the treatment, or rather when and how to operate, and I will demonstrate to you my method of operating.

S. W. Cor. Salisbury St. and Broadway, St. Louis, Mo.

ABSORBENT COTTON AS A DRESSING.

By Wm. D. Babcock, M.D., Evansville, Ind.

For over a year I have been using absorbent cotton as a dressing for the umbilical cord. I have been pleased beyond my expectations. I have had during that time not a single bad result. The cord coming off clean and smooth, not even the sign of a granulation. The skin around was not irritated in the least. My position as attendant to the Christian Home here, a home for unfortunate girls, gives me a good opportunity to try it. Even with the inexperienced girls as nurses I have had no bad effects. When the child is ready to be dressed, I take a liberal piece of the cotton and envelope the cord well. I use no grease of any kind. Dress the cord only once. Do not allow the dressing to be disturbed by the nurse looking at the cord each time the child is dressed. I thus prevent all pulling on the cord. The cord comes off in from three to four days, leaving a perfect navel. There is not at any time the slightest odor. I have used it in some twenty cases. I hope the members of the profession will give it a trial. It will, I think, in a number of cases prevent umbilical hernia by preventing pulling, which occurs more or less in every case when cloth is used. It also gives gentle and equal presssure over the navel.

POP-CORN IN VOMITING OF PREGNANCY.

By T. C. Smith, M.D., Aurora, Ind.

I have just seen Dr. Sweringen's highly favorable report of the value of pop-corn, in the nausea and vomiting of pregnancy. A very severe and troublesome case of the kind came under my care in March and April, of this year, most distressing and dangerous too. I used about everything books or experience commended, pop-corn included. Very little benefit followed the use of any remedy or means resorted to and none came from the free use of pop-corn. After the fourth month there

was no further trouble. Pop-corn may benefit a few cases, but like most all remedies it is sure to meet with its failures.

Society Reports.

OBSTETRICAL SOCIETY OF LONDON.

Stated Meeting, July 4th, 1883.

The President, H. Gervis, M.D., in the Chair.

Dr. Champneys read the following paper on

THE OBSTETRICS OF THE KYPHOTIC PELVIS.

An analysis was given of thirty-two labors in twenty women, including three labors in a patient of the author's, the last labor having beeen carefully observed. An analysis and a table were given stating the presentation, change during labor, measurements of fœtal skull and pelvis, operative measures, moulding of fœtal skull, result to child and mother. The general remarks of other writers on the subject were summarised. The general conclusions at which the author arrived were the following. Vertex presentations, and especially right occipito iliac positions, are unusually frequent; deep transverse position is common, posterior rotation not uncommon. The comparative frequency of occipito-posterior positions is probably due (as explained by Hoening) to the obstacle to forward rotation in third positions, which are very common. The head sometimes emerges from the ligamentous pelvis transverse or nearly so, and entirely posterior to the tubera ischii. The analogy to the "extramedian" position was pointed out. The well known looseness of the pelvic joints in this pelvis probably assisted this by the nutrition of the sacrum. Spontaneous premature labor is not uncommon. The immediate fœtal mortality in the published cases was 40.6 per cent., the maternal 28.1 per cent.; but the author thought this estimate too high, as slight cases were not recorded. The conclusions as to treatment and prognosis were these.

1. In a first labor, if the head present, wait and act according to circumstances. This implies forceps, craniotomy, or Cæsarean section, which should always be considered in the above order.

2. If the head present, never turn.

3. In the subjacent labors, where the history of the first labor seems to indicate it, premature labor may be induced with good hope.

4. No known measurements give us any sure indication for forceps, turning, Cæsarean section, or the date for induction of premature labor.

5. The mobility of the pelvic joints implies a prognosis always more favorable than measurements would lead us to suppose.

6. Probably in many cases the head entirely neglects the anterior half of the pelvic outlet, and emerges from it transversely, or at most obliquely; antero-posterior emergence being the exception.

7. Each succeeding difficult labor increases the liability of the uterus to rupture as in other forms of pelvic distortion.

Dr. Roper remarked that the mechanism described by Dr. Champneys resembled that of labor in the lower animals, in which there was no pubic arch, and the fœtus always passed behind the ischial tuberosities. This diminution of the curve in the pelvic axis somewhat lessened the difficulty of labor. In cases of kyphosis the vertical capacity of the abdomen was diminished, hence the uterus was thrust forward, and pendulous belly was common, and led to difficulty in the entry of the fœtus into the brim. He described a case which he had seen. In these cases the deformity of the outlet obstructed delivery more than that of the brim.

Dr. Herman agreed with the author that published statistics probably contained an undue proportion of difficult cases.

Dr. Champneys thought that pendulous belly was produced by anything which shortened the abdominal cavity.

A Note on Uterine Myoma, its Pathology and Treatment.

Mr. Lawson Tait thought that the word "myoma" should entirely supersede the incorrect term "uterine fibroid." The growth of ordinary myoma was limited to the period of sexual activity, was influenced by the menstrual function, and probably its ultimate cause would be found in some disturbance of the nervous body which governed that function.

The presence of a myoma indefinitely delayed the menopause. Menstruation and ovulation, he thought, were completely independent functions, having perhaps a community of purpose. Removal of the ovaries often did not affect menstruation, but removal of the pubes nearly always did so. But in one case in which he had removed the ovaries, tubes, and part of the fundus uteri, menstruation continued for more than a year. He deprecated the triple subdivision of myomata into submucous, intramural, and subperitoneal. For pathological and surgical purposes, he proposed a new subdivision into the nodular and the concentric. The latter consisted of a uniform hypertrophy of the muscular tissue of the uterus, in the midst of which the canal lay centrally; the tissue of this form was loose, and usually very œdematous. Of the nodular myoma he proposed two sub-varieties, the simple and the multi-nodular. He believed that each nodule was seated in a central arterial twig, and that its growth was endogenous, the older tissue being on the outside. The dependence of such growth on menstruation was proved by the fact that arrest of menstruation arrested the growth, or even caused the complete disappearance, of such tumors. This had been in several cases brought

about by the removal of the Fallopian tubes only. He had treated 54 cases of uterine myoma by removal of the uterine appendages, with 3 deaths, a mortality of 5.5 per cent., a striking contrast of the results of hysterectomy. Of these 51, in 38 the results had been carefully followed, and were everything that was to be desired. In 3, the tumors were, or became, malignant. In 3 others, the tumors continued to grow, although menstruation had been arrested. The author suspected that these were either fibro-cystic, or myoma of the concentric variety, in neither of which forms was the removal of the uterine appendages useful.

The President was hardly prepared to accept Mr. Tait's classification, but it was not necessarily antagonistic to the one in common use. He agreed with Mr. Tait as to the delay in the menopause in these cases. He would like further evidence as to the sole or even large influence of the tubes in the phenomena of menstruation.

Dr. Herman had published a case in which the symptoms of a fibroid polypus first appeared sixteen years after the menopause. The history of patients after operations like those of Mr. Tait, was of great importance, for patients not benefitted often did not return to the operator, and he therefore was apt to get a too favorable impression of the results.

Dr. Dewar asked if Mr. Tait was careful to tie the uterine artery; and whether removal of the Fallopian tubes, leaving the ovaries, was not dangerous. He had seen one case in which the uterine appendages had been removed, and hysterectomy was subsequently required on account of hæmorrhage.

Dr. Meadows preferred the present classification of fibroid growths to that suggested by Mr. Tait, as being founded on clinical characters, and of great practical value for diagnosis and treatment. He believed that the ovaries, and not the tubes, were the prime movers in menstruation. In one case he had removed the overies and left the tubes, and menstruation ceased. He thought there were many exceptions to the rule that uterine fibromata ceased to grow after the menopause. Notwithstanding the high rate of mortality which attend hysterectomy, he preferred it to the removal of the ovaries.

Mr. Lawson Tait said that cases of growth of apparent uterine myomata after the menopause needed most careful examination. Occasionally removal of the ovaries arrested menstruation, but this was the exception. He had never knowingly tied the uterine artery, and it would be very difficult to do so.—*British Medical Journal.*

Existence of Syphilis in America before the discovery by Columbus.—Dr. Whitney discusses this question in the *Boston Med. and Surg. Jour.*, April 19, 1883, and is of opinion that the evidence presented thus far does not prove the existence of syphilis in America previously to the landing of the Spaniards.

BRITISH MEDICAL ASSOCIATION.—PROCEEDINGS OF OBSTETRIC SECTION.

On Metria (So-Called Puerperal Fever.)

By *Lombe Atthill, M.D.*, Ex-Master of the Rotunda Hospital, Dublin.

Your Secretaries have requested me to initiate a discussion on the subject of Metria—a term which includes a number of affections which, though allied in some respects, are in others very dissimilar, but all of which are commonly classed together under the very incorrect title of "puerperal fever." I admit the force of their argument, that no more important or practical subject could be brought forward for discussion here; but I hesitated to accede to their wishes, because it is very recently that I read before the Obstetrical Section of the Dublin Academy of Medicine a paper on metria, which has already appeared in print, and from which my present communication cannot possibly materially differ. But, as your secretaries still considered it advisable that I should bring the subject under the notice of this section of the British Medical Association on the occasion of its annual meeting, I consented to do so.

The pathology of metria is still far from being perfectly understood. Two facts alone are admitted by all who have studied the subject carefully; namely, first, that puerperal women are liable, under certain circumstances, to be inoculated with septic matter conveyed to, and deposited in, the vagina by the hands of the attendants, as well as by other agencies, when, either through carelessness or ignorance, proper precautions have not been adopted to prevent such an occurrence; and that the disease produced by such inoculation is not an unfrequent source of one of the forms of metria; secondly, that puerperal women may be self-inoculated by poisonous matter originating within their own bodies, from the decomposition of blood-clots formed within the uterus after parturition, or of portions of the membranes or placenta which have been retained *in utero*; the only difference of opinion on this point being, that Dr. Matthews Duncan and others term the disease thus produced "sapremia"—that is, resulting from the absorption of putrid matter—thus distinguishing it from "septicemia," or the disease produced by "organisms which, when conveyed to the blood, multiply indefinitely in it;" while those which are the product of putrefaction "do not survive, far less grow, therein." (Dr. Matthews Duncan on Puerperal Fever, *Lancet*, Nov. 6th, 1880.)

I hardly think that anyone will dispute the correctness of the foregoing points; they have been established beyond all doubt; and it is certain that poison, introduced into the system by one of the two ways indicated, is the cause, in the vast majority of cases, of so-called puerperal fever, whether occurring in private or hospital practice. But there are many

who believe that the whole subject is summed up in a belief of these very important propositions, and who think that to go outside these lines is only to cause difficulty and to create confusion. I admit this; but it seems to me that such an argument is almost an appeal *ad misericordiam*, and that it cannot be admitted for a moment. I believe that, in addition to the two preventable forms alluded to above, we have others; and I ask the members of this Section to consider whether we have not, in addition to these, two other forms of metria, which it may not be possible to guard against—namely:

1. A form of self-infection, under special conditions, to which I shall allude by-and-bye, which is not preventable by the adoption of any antiseptic treatment;

2. An epidemic, highly infectious, form, which spreads by the same means as ordinary epidemics do.

Before commencing the discussion of these propositions, it is essential to bear in mind that I entirely concur in the opinion now generally held, that septicemia, occurring in a puerperal woman, is not capable of being communicated to another puerperal patient by any means other than the direct transfer of the infectious matter to some portion of the mucous membrane lining the genital track. Septicemia, however, when it attacks a puerperal woman, may be spread by various agencies, as well as by the hands of the attendant—for instance, by the nozzle of a syringe, by the use of infected sponges, by imperfectly washed napkins, bed-linen, etc.; but not through the medium of the air breathed by the patient. Of the truth of this I have not the slightest doubt.

You will observe that I have spoken of the two ordinary forms of puerperal septicemia as being preventable. It is evident that, with thorough cleanliness, and the use of antiseptic precautions, septic poison should never be introduced into the patient's system by the attendants; further, I believe it is possible, to prevent self-infection in a healthy woman, by adopting precautions to insure a good and permanent contraction of the uterus, and by washing out the uterus whenever we have reason to suspect the existence of clots, etc., in it, with a disinfecting fluid. With the former object, I make it a practice to put all patients in whom a relaxed condition of the uterus exists, on ergot, from the moment labor terminates, continuing its administration for at least a week. I believe a relaxed condition of the uterus to be a very common predisposing cause of self-infection in puerperal women; it favors the formation of clots in utero, and also, the orifices of the uterine sinuses being left open, the absorption of septic matter is favored.

In proof that I do not exaggerate the importance of imperfect contraction of the uterus, as a main factor in the production of puerperal septicemia, I may point out that I recently saw, in consultation, three patients suffering from this affection, in all of whom labor had been so

rapid that the child was born before the arrival of the medical attendant; and, it is a well-known fact, that relaxation of the uterus is very liable to follow the too rapid emptying of that organ.

This train of reasoning had led me to believe that imperfect uterine contraction is one of the causes of the frequent occurrence of septicemia in unmarried women. The mortality from septicemia amongst them is very great, and there is no doubt but that the great mental distress these poor creatures suffer, interferes with the recuperative process which should take place rapidly in the uterus after parturition. The muscular fibres of the organ do not contract as they should; the blood-supply, consequently, is not cut off, the mouths of the sinuses remain open, the denuded placental site, instead of becoming rapidly restored to its normal condition, becomes unhealthy, and the fetid discharge, which, under the circumstances, takes the place of the normal lochia, enters the system, either directly through the open mouths of the placental sinuses, or is absorbed at the site of some fissure in the mucous membrane lining the genital track. This is one form of puerperal septicemia which I fear is beyond the reach of preventive treatment. No antiseptic precautions can prevent its occurrence, no treatment that I know of will stay its progress. In patients suffering from certain forms of chronic disease a similar condition is observed, and similar results follow.

In my opinion, the infection arising from any of the forms of metria to which I have alluded, cannot be carried by the attendants from one patient to another, if precautions be adopted to prevent it. And only a year ago I was strongly inclined to believe that epidemics of so-called puerperal fever would not occur so long as such precautions were adopted. Those enforced by me among the pupils attending the Rotunda Hospital were the following:

1. Students attending the practice of the hospital should not undertake post mortem examinations, be engaged in dissections, or attend an hospital containing patients suffering from infectious diseases; and

2. Before proceeding to examine any patients, they washed their hands in a solution of carbolic acid.

During the first six years and a half of my mastership, these sufficed to prevent the occurrence of anything like an epidemic of so-called puerperal fever. Deaths from septicemia, especially among unmarried women, from time to time occurred, but the disease never spread; in August last, however, the hospital being at the time extremely healthy, a patient was admitted who complained of pain in the abdomen, and who vomited constantly, the fluid ejected being greenish. She stated that she had been in labor more than twelve hours, and that, during the whole of that time, she had been vomiting; and it was subsequently elicited that she had been complaining for some days previously, and also that she had been seen, at the commencement of labor, by some practitioner,

who advised her to go into hospital. The os, at the time of admission was about one third dilated, labor progressed very slowly, and was finally delivered by the forceps. Vomiting ceased after delivery for a time, but soon recurred, everything swallowed being ejected, with large quantities of greenish fluid. The abdomen became tympanitic, the pain intense, matters went from bad to worse, and she died on the fourth day after delivery. Her appearance strongly resembled that of a patient suffering from typhus fever.

Another patient was admitted on the same day as the last patient and she lay for a short time in the bed next to her. The patient's labor also was slow, but it terminated by the natural efforts. She was attacked with symptoms of acute peritonitis thirty-six hours after delivery, and almost immediately afterwards we noticed a very peculiar, almost black, appearance of the face. The course of the disease was identical with that of the preceding case, but was even more rapid. The first symptoms showed themselves on the morning of the 29th, and she died on the 31st.

The disease now spread rapidly, and so virulent was the epidemic that, out of twenty-nine women admitted during six days which intervened between the delivery of the first patient and the issue of the order to refuse admission to all applicants, eleven women were attacked, and nine died.

The admission of patients being stopped, the wards were thoroughly disinfected, the walls lime-washed, the floors washed with a strong solution of chloride of lime, the cupboards, presses, etc., scoured, the nurses' clothes, as well as their bedding, being washed and aired, and placed in the hot-air chamber. Patients were re-admitted on September 12th; and from that date till the expiration of my mastership on November 4th, during which time 118 women were admitted into the hospital, the health of the patients was excellent, and I am informed, continues to be so still. No more successful effort to stamp out disease than this was ever recorded. This, and the fact that the epidemic was distinctly imported into the hospital, and that it did not originate in it, are facts as important as they are satisfactory: and though the occurrence of the outbreak was a cause of great distress to me, and though it was a great disappointment that, at the very close of my mastership, such a misfortune should have happened, still these two facts lessened the regret I naturally experienced.

Some years previously, a patient suffering from erysipelas of the head and face was, during the night, sent up to the labor-ward, her condition not having been detected till she was being undressed. The child's head was in the perineum, and she could not be sent out. She was at once removed to a separate ward, and early next morning transferred to a fever hospital; but though her stay in the Lying-in Hospital was so

short, several patients were attacked, not with erysipelas, but with so-called puerperal fever, and one died. The disease was limited to the one ward. I ask you, gentlemen, to consider what the disease attacking these women was. To me, it seems to have been a disease originating by the introduction into the system of a puerperal woman of the infection of erysipelas, which infection was modified by the peculiar state of the system and of the blood which exists in puerperal women, and which, therefore developed an apparently different disease; and I am strongly inclined to the belief that outbreaks of so-called puerperal fever, when it assumes an infectious and epidemic form, are due to the introduction of the poison of some ordinary zymotic disease into the system of a puerperal patient, the symptoms being, under such circumstances, totally different from those occurring in cases of septicemia.

Remarks on Metria.

By *John Thorburn, M.D.*, Professor of Obstetric Medicine in Owens College, Manchester:

I had no intention of bringing the subject of Metria before this section until I learned that it was about to be introduced as one of the special subjects for discussion. One or two points in connection with it I had hoped to bring before the Obstetrical Society, in order to obtain some expression of opinion upon them. This occasion, however, seems to furnish an equally good, if not superior, opportunity.

In March, 1881, I ventured to make some remarks on the subjects of metria to the Manchester Medical Society, which I published and forwarded to many gentleman now present. Following in the lines of an address delivered by Dr. Matthews Duncan at Birmingham in 1880, I adopted the views now almost universally held, discarding altogether the idea of a distinct and specific puerperal fever, and considering the symptoms so familiar to us all after delivery as having their origin in more than one separate cause. The conditions of the puerperal system is a strong tendency for the symptoms, however diversely originated, to assume a somewhat definite and uniform character, still designated by most of us as "puerperal fever." The ordinary forms of sepsis and zymosis are here at work in a soil specially prepared to influence them by the blood-changes of pregnancy and labor.

I will not discuss the special part played by each of these causes, by pyæmia, septicæmia, sapræmia, and the like, as I feel sure this will be done more ably by others, but I will continue myself to three moot points.

1. The position of the zymotic diseases as a frequent cause of metria.

2. The difficulty in including epidemic "puerperal fever" in the same kind of classification as the more sporadic forms.

3. The advisability of adopting the term metria, and abandoning alto-

gether that of puerperal fever. On the last point, it would be of great importance to obtain, as far as the rules of the Section permit, a collective expression of opinion.

1. As regards the share taken by the zymotic diseases in the production of symptoms wholly characteristic of the puerperal state, there is much confusion of thought and expression. That in some way the poisons of scarlatina, or erysipelas, or typhoid fever may sometimes cause in puerpera symptoms scarcely distinguishable from those of puerperal fever, is hardly now disputed, even by those who still hanker after the exploded idea of a fever of that name which is strictly *sue generis*; yet one so far removed from this idea as Dr. Duncan, does not include zymosis along with septicæmia, or in his list of causes.

On the one hand, every obstetric practitioner must have seen cases of typical, and even mild scarlatina, running their course unaffected in any way by the coexistence of the puerperal state. I have seen intense phlegmonous erysipelas of one limb in a recent puerpera, who, beyond the moderate rise in temperature, showed no other metrical or puerperal symptoms, and suckled her child successfully throughout. (1)

On the other hand, who has not seen cases of puerperal fever, if ever there were such a thing, in its most typical and fatal form, where the sudden rigors, intense tympanites, high temperature, facial expression, rapid death, with or without colliquative sweating or diarrhœa, were only guides to diagnosis; while the subsequent clear proof of scarlatinal infection of the patient, or the handing on of it to nurses or relatives, or even, more rarely, the occurrence of specific rash in the hour of death, have clearly stamped the disease as metria of scarlatinal origin? Such cases are usually hushed up, but the consulting physician sees too many of them.

If puerperal fever and metria are to be used as collective terms at all, I do not see how, from a practical point of view, we can avoid including zymotic causation. A puerpera who has scarlatina which can be distinctly diagnosed, has scarlatina, and there is an end of it; but a woman who has the symptoms roughly described above, has puerperal fever, or metria, or whatever we are compelled to call it; and scarlatina, or drain-poison, or erysipelas may, for ought we know, be its cause.

2. As regards epidemic puerperal fever, many who adopt the modern views as to ordinary sporadic cases of disease, hesitate to apply them to epidemics, where numerous cases are met with in rapid succession, or side by side, and when the cases are all of the same severe and fatal character. A further knowledge than we yet possess of how disease-germs propagate, or, in certain circumstances, become hybrid, or how

1 Several cases of erysipelas in conjunction with puerperism, but not causing metrical symptoms, are recorded in the *American Journal of Obstetrics* by Campbell and others; and Hungenberger, in the *Archiv f. Gynäkol.*, vol. xiii, p. 387, mentions many attacking the genitals; but there seems to be an agreement, that, in all these cases, the phlegmonous character was absent.

their activity becomes attenuated or intensified, will probably make this matter plain enough; but, meanwhile, no one believes that sporadic hospital gangrene, or erysipelas, which can often in ordinary practice, be traced to its real sources, is a different disease, a disease more really *sui generis*, when it attacks almost every patient as at the seige of Lucknow, or in the trenches before Sebastopol. Precisely the same causes—and there is seldom only one cause—are at work; and if they occasionally produce an epidemic with remarkable uniformity and severity of symptoms, it is exactly analogous to what occasionally happens in a lying-in hospital, or, more rarely, in individual practice. From this point of view, and it surely cannot, nowadays, be considered a purely theoretical one, there would seem to be no difficulty in conceiving how that tolerably definite train of symptoms which ensues from the action of septic or zymotic germs in the hyperinotic blood of a puerpera may occasionally become stamped with the characters of absolute uniformity and epidemic virulence. The wave passes over, as those of all epidemics do, and we return to sporadic cases. Time will not permit me to further work out this.(1)

3. We have, lastly, the question of nomenclature, in many instances a very important one. If we are agreed that puerperal fever, meaning thereby a specific fever, totally distinct from the ordinary sporadic cases of septic or other poisoning, is non-existent, what are we to do with its name? Are we, as common sense would, I think, suggest, to abandon it altogether? Or are we to use it for the purpose of denoting something else which involves a totally different form of ideas, and the belief in which ignores or deprecates any such thing as a specific fever? Or are we, as is practically done on all hands, to call our apparently desperate cases puerperal fever, and to carefully abstain from giving the title to cases which seem likely to recover, or when the name would be dangerous to our own practices? Those who are in general practice, will agree with me that considerations of this kind do often bear on the use that is made of a term which has ceased to have an accurate scientific meaning, but which has not ceased to have a very serious popular one. Why should we retain the term at all, except those of us who are still prepared to defend the pathology involved? Every argument of which I can think points the other way. In some instances, there is an obvious advantage in retaining terms which have lost their original meaning, but which still do convey something to the mind that is universally agreed upon. But this is not the case here. To one man the name conveys its original meaning, and thereby serves to perpetuate a fallacy; to another

1. It would be inexcusable not to make some reference to the very able papers of Dr. Napier, in the *Obstetrical Journal* for 1880, on puerperal fever. He, along with Dr. Fordyce Barker, defends the old position of a specific puerperal fever *sui generis*. The time-limit imposed by our regulations, allows no opportunity of consulting, step by step, such arguments as he adduces. I can only say that his invaluable collection of facts produces in me an opinion diametrically opposed to his own.

it may have the more harmless meaning of a collective term for nearly all the febrile affections of the puerperal state. To the general public it means, and will for many years continue to mean, death and despair. It is possible, of course, to draw some kind of boundary at which puerperal fever commences, and ordinary septic inflammation, or fever, ends, just as one might place rheumatic fever on one side of a line, and acute rheumatism on the other; but surely, if this latter distinction were practically one of almost life and death, we should all consider it as discredited and condemned.

If, then, we are to give up puerperal fever, are we in a position to do without any collective term? Are our ideas as to the various forms and results of blood-poisoning so advanced, that we can now say definitely, what I hope we always try to ascertain: "This is a case of pure inflammation of this or that organ, independent of any sepsis; this is a case of septicemia, of a common surgical type; this is a case of purulent absorption, and that of zymotic poisoning?" I am bound to confess that I should be compelled to change my diagnosis and terminology, in many cases, almost from day to day; that what appeared to me at first to be a peritonitis, might turn out to be a typhus or scarlatina, or might leave strong suspicions that it was so.

If, then, we must have another collective term, what is there better than metria? It is ready to our hand, and is simple and easy to pronounce, which, in these days of barbarous Græcomedicalese, is something in its favor. It was strongly recommended by Dr. Farr, that unrequited master of vital statistics. It was adopted by the late Registrar-General, and is already incorporated in our national health history books. It is sufficiently indefinite to serve the purpose of a provisional and collective term. The public will soon understand by it all that is required—viz., a critical form of febrile or inflammatory attack, to which puerperal patients are liable, but which depends on many causes, against which they and we must guard. They will build their hopes or fears, not on a mere name, but on our interpretation of the individual symptoms. The profession will recognize it as conveying no exploded views on pathology, but as leaving an open door for newer ones. They will consider it as a generic term for symptoms, common in their character and confined to the puerpera, though allied to symptoms otherwise modified in the surgical ward; and demanding, in every case, the earnest study of the practitioner as to their causation, prognosis and treatment.

Abandoning the term puerperal fever, until the pathology of blood-poisoning is further advanced, we can only choose between metria and chaos.

If our rules permit such a thing, and if there be anything like unanimity on this point, it would be of much service if we could give forth

some collective recommendation. Its general adoption would save a world of trouble and misunderstanding.

I trust that this concise, and perhaps somewhat dogmatic, treatment of the three points thus raised will be taken as holding out topics for discussion, rather than as any attempt fully to discuss them.

Observations on Puerperal Fever.

By *Thomas More Madden*, M.D., F.R.C.S.E., Dublin, Obstetric Physician to the Mater Misericordiæ Hospital.

Puerperal fever, or metria, has always been a favorable subject of obstetric controversy, and we should, I think, be much indebted to Dr. Thorburn for so ably introducing the consideration of perhaps the most important practical question that can be here discussed; for, as long as we have one death in every five hundred deliveries—in other words, that 5,514 women die annually in this country after childbirth from an infective disease consequent on parturion—it cannot be unimportant for us to discuss its prevention and treatment (Annual Report of the Registrar-General for 1882.)

It would be obviously impossible, within the very limited time at our disposal, to give any exhaustive or adequate attention to so vast a subject as this. We may, however, each, from our own clinical experience, say a few words on those points which appear to us of great practical importance.

According to the annual reports of the Registrar-General, no fewer than 44,306 deaths from metria were registered in England between the years 1847 and 1881 inclusively. To these must be also added 120,140 fatal cases registered within the same time, and tabulated in another column as resulting from "childbirth and metria." In other words, 164,446 deaths from septic puerperal disease have occurred in this country in shorter period than the lifetime of many of us who are now discussing its prevention and treatment.

Nor in Ireland has the mortality from this cause been less alarming than in England. Thus, we learn from the registration reports that, during the past ten years, about 3,300 deaths from metria have been registered in Ireland. These figures, however, I think by no means represent adequately the actual mortality from puerperal fever in either country. The majority of parturient women, especially in country districts, have no skilled medical assistance during labor, being then too generally dependent on the good offices of some friendly neighbor or midwife. Under such circumstances, when death from metria occurs, we can hardly expect any accurate statement of its true cause. And probably the very last cause that would willingly be assigned by an ignorant or unscrupulous person for the death of anyone under his or her care after delivery would be puerperal fever.

The statistical reports just referred to might also be cited, did time

permit, to prove another fact of great practical importance, viz., that notwithstanding the unquestionable advance which, within the past few years, has taken place in the obstetric art, puerperal fever or septicæmia is now as at any former period. And we are apparently still as far as ever from that promised "stamping out" of puerperal fever which has been prognosticated so confidently by some obstetricians as the result of the recently improved methods of delivery and antiseptic treatment of puerperal patients. But, on the other hand, we have certainly progressed materially of late years in the treatment of all puerperal diseases, and more especially septicæmia or metria.

Pathology of Puerperal Fever.—Dr. Atthill's views on this subject appear to have been generally adopted by other recent authorities, many of whom now agree with him in believing that, in the first place, there is no such specific disease as puerperal fever—that is, a specific disease in the same sense as small-pox or scarlatina; secondly, that inoculation or absorption of septic matter from without is a frequent cause of one form of metria, i.e., puerperal septicæmia; and, thirdly, that puerperæ frequently become self-poisoned by auto-infection from decomposition of retained clots, or shreds of membrane or placenta; the result being what some call puerperal sapraemia, in contradistinction to septicaemia. These are Dr. Atthill's views; and his experience has been so great, and his authority is so high, that it is with great regret I find myself obliged to dissent, as I do, *toto cælo*, from the first of them, and to some extent from the others also. We are, however, met here to arrive at accurate conclusions on debatable questions, by the exchange of our individual experiences and views. Hence, having been for upwards of twenty years in practice, and having been for some years connected with the largest lying-in hospital in Great Britain, I have had some opportunity of gaining experience on this subject. I have, therefore, no hesitation in saying that, in common with others who have had similar experience, I am as convinced as I can be of any fact whatever of the existence of puerperal fever or a specific infectious disease peculiar to puerperal women. The entity of this disease is in no way affected by whatever name we may choose to term it; and whether we speak of it as puerperal fever, metria, septicaemia, utero-peritonitis, sapaemia, or by any other appellation, its distinct existence remains as unmistakable as that of measles, scarlatina, typhoid or typhus fever, or any other zymotic disease; although its predominant symptoms are varied, as those of these diseases also are, at different periods, and during different epidemics, by the prevailing atmospheric epidemic constitution, by the general condition of the patient, by the intensity of the septicaemic intoxication in each case, and by a variety of other modifying circumstances.

In considering a malady of such gravity as this, any needless subdi-

visions or hair-splitting pathological distinctions are, I think, misplaced, and are rather calculated to embarrass than to assist the practitioner who has to deal practically with the different aspects of the disease at the bedside of the puerperal patient. Hence I think we should still retain the old name of puerperal fever, as most generally applicable, and conveying no debatable theory.

Causes of Puerperal Fever: Infection, Auto-inoculation, Hospitalism, etc.—With regard to the causation of puerperal fever, several much disputed questions present themselves. For my own part, I regard puerperal fever as a zymotic infective disease prevailing periodically as an epidemic, and being, moreover, endemic in some places, under certain circumstances.

It is unquestionable that the disease may result from infection with the poison of other zymotics, such as erysipelas, scarlatina, and typhus fevers, as well as be induced by auto-inoculation with septic matter self-generated in the patient's system, or by hetero-inoculation with septic matter introduced from without. Dr. Burdon Sanderson's views with regard to the gradually increasing virulency of successive inoculation with the exudations of peritoneal inflammation, have been gradually accepted by pathologists. According to this doctrine, if the exudation of a simple peritonitis be injected into the peritoneum of another animal, the disease assumes a more active form in the second than in the first instance; and so on gradually develops its intensity, until at last a virus is reached of the virulence of malignant peritonitis in the human subject. This gradual evolution from traumatic infectivity to the intensified virulence of malignant septicæmia, concludes Dr. Sanderson, teaches plainly what we could not otherwise learn.

The same view is also deducible from clinical experience. Thus, in the Dublin Lying-in Hospital, before any epidemic outbreak of puerperal septicaemia, a few sporadic cases of puerperal fever are noticed to occur, at gradually shortening intervals between them. These cases are usually at first of a mild type, and most of the patients primarily attacked recover. Soon, however, the number of the cases and the virulence of the disease increase, until the hospital at last becomes the hotbed of epidemic puerperal septicemia, from which recovery is exceptional and which cannot be arrested until the institution has been closed for some time.

It would, therefore, be impossible to glance, however briefly, at the etiology of puerperal fever, without some reference to the alleged influence of lying-in hospitals in the epidemic spread of this disease. For many years I doubted that these hospitals had any bad influence; and, in former discussions on this subject, I took part in maintaining that even the largest institutions in which lying-in women are aggregated could be, by proper hygienic management, conducted without any fear

of their ever proving centres of epidemic puerperal disease; and I only regret that, with my present experience, and from a full review of the facts bearing on the subject, I can no longer hold this view. The mortality caused by puerperal fever in the population at large, great as it is, amounting, as I have already observed, to about one death in every 500 deliveries, is fearfully exceeded in all special lying-in hospitals, in which large numbers of puerperal women are aggregated together. In these institutions, the germs of septicemia are developed with a rapidity and attain a virulence unknown under any other circumstances. Thus, in the two Dublin lying-in hospitals, the mortality from puerperal fever is now five times larger than in extern deliveries in the population at large throughout England, as shown by the last report of the Registrar-General and of the Hospital Board of Superintendence.

There is unquestionably much to be said in favor of large lying-in hospitals, such as that admirably managed institution the Rotunda Hospital, which is an enduring monument of the self-sacrificing charity of its founder, a member of our profession, and is the justly revered Alma Mater of the Dublin School of Midwifery, which has done so much for the progress of obstetric science. But vast as are the benefits that have been conferred by great maternity hospitals such as this on innumerable women in their hour of direst trial; countless as may have been the lives therein saved from all the emergencies of childbirth; great as is the utility of such institutions as centres of obstetric education; yet all these considerations are, I fear, outweighed by the fact that puerperal fever therein finds its favorite *habitat*, and that its virus radiates thence at certain intervals in outbursts of epidemic malignant puerperal septicemia.

Amongst the causes of puerperal fever, some reference should be made to laceration, during labor, of the cervix uteri. This accident, especially where the injury has been occasioned by the abuse or premature employment of the forceps before the natural dilatation of the os, is probably a very important, though generally entirely unrecognized, factor in the modern etiology of puerperal septicaemia. Under such circumstances, the danger of rupturing the undilated parts is self-evident. And it is equally obvious that thereby is afforded a ready channel for the auto-inoculation of the patient with any septic poison existing in the lochial discharge, which may be absorbed through the raw edges of the lacerated surfaces, and thus give rise to septicemia. There can be no question as to the toxic effect of inoculation with even apparently healthy lochial matter; and, therefore, much more likely is this to occur when the lochia are in an abnormal or vitiated condition, as is so generally the case a few days after difficult and instrumental deliveries.

Treatment.—There can be no question of the general fatality of epidemic puerperal fever; but still, we have improved considerably in

our treatment of this disease, as shown by its result. It is not so very long since an eminent obstetrician reported that he would quite as soon be called to a case of hydrophobia as to one of puerperal fever. I remember hearing the late Dr. Stokes, in the course of a discussion on this subject, saying that, during an extensive practice, extending over forty years, he had very frequently been called in consultation to cases of puerperal fever in the later stages, and that, during this long period, he had never seen a single instance of the kind in which the patient recovered. Now, I am sure that there is no practitioner present who, thanks to the recent progress of obstetric medicine, has not been enabled to treat successfully at least some of those cases which, only fourteen years ago, proved uniformly fatal in the practice of one of the most eminent physicians of his day.

The treatment of puerperal fever must be governed by the predominant symptoms of each case, and depends largely on the prevailing epidemic type of the disease, which varies widely at different times. Even within the comparatively short period included in my own obstetric experience, several changes have taken place in the prevailing type of puerperal fever in successive epidemics, and hence in the treatment required. We now seldom, if ever, meet with the true inflammatory utero-peritonitis, for which, in my student days, mercury with opium, and free depletion by leeching, were almost invariably prescribed. I well remember often seeing the puerperal patient's abdomen covered, under such circumstances, by what the late Dr. McClintock graphically described as a poultice of leeches. And I may add, that I have still a lively recollection of the benefits derivable, in appropriate cases, from this line of treatment. Within the last fifteen or eighteen years, however, I have never seen a case of puerperal fever in which any form of depletion could be tolerated; the disease having now, in common with all others, assumed an asthenic, or typhoid, form, and like them appearing more in the character of a septicemic, than of a true inflammatory malady.

Thus, when, some years after my first acquaintance with the practice of the Rotunda as a student, I became one of the medical staff of the same hospital, two forms of puerperal fever came before me; one with marked uterine pain and tenderness and abdominal distension, and the other without any localized pain; both accompanied by a low typhoid condition tending to death, and obviously requiring stimulation, and especially the free use of turpentine by the mouth, by enemata, and by external application in stuping the abdomen. The form of puerperal fever now most frequently met with is distinctly remittant in its type. Several cases of this kind have come under my observation in which the fever was of the tertian character. Still more usually, however, there are daily matutinal remissions. Thus the temperature and pulse in the

second week of the illness often fall each morning to little above normal, and again rise throughout the afternoon, until in the evening the former has reached 105°, and at the same time the pulse becomes above 120.

In the treatment of the remittent forms of puerperal septicemia, our main reliance must be placed on quinine. This should be given in medium doses of from three to four grains at short intervals of three to four hours, and continued until the pulse and temperature have been sufficiently reduced, and cinchonism has been maintained for some days.

I may repeat that, with very few exceptions, all the cases of puerperal fever I have recently seen were of an essentially asthenic type, presenting all the symptoms of so-called malignant puerperal fever or septicæmia, and, consequently, were not suitable cases for any form of depletion; but, on the contrary, required the free use of stimulants and nutriment.

Whatever other treatment may be indicated, however, the use, twice daily, of antiseptic intra-uterine and vaginal injections is essential in every case of puerperal septicæmia. The use of such injections, for the purpose of thoroughly washing out septic exudations from the cavity of the uterus, is self-evident. But, at the same time, it should be said that they require to be used with far more caution than is generally practiced. Nor should we ever fail to impress on the nurse, in such cases, the risk of probably injecting into the open uterine sinuses; or, on the other hand, of forcing the injected fluid through the patulous Fallopian tubes. I have more than once seen injury caused, in both these ways, by want of such caution in the use of the ordinary siphon syringe.

M. F. J. Bailey (Liverpool) said that there was no doubt at all that, with the advance in the obstetrical art, the term puerperal fever—which, in olden times, was commonly used—ought not now to be used to embrace all those forms of feverish disturbance that were met with. A condition of feverish disturbance was, he believed, induced in all women by the want of proper nourishment, and the absence of sufficient of the nitrogenous element in food; and the poisons induced by the presence of scarlet fever and diphtheria ought to be classed under their distinctive names. Those cases of fever following septicæmia from putrid clots or small portions of retained placenta inducing alarming fever and inflammation of a characteristic kind, should also be classed under their generic name. Then again, there was pyæmia, from pus floating about. There was the condition known as surgical fever, arising from the shock to the system. The use of forceps did not induce it, nor did a torn perinæum; in many, the early use of the forceps would prevent it. Great cleanliness and antiseptics were no doubt absolutely necessary.

Dr. Fancourt Barnes (London) had for some years, from his connection with the Royal Maternity Charity and the British Lying-in Hospital, seen many cases of puerperal fever. He was content to retain the

term puerperal fever, as fairly describing that condition in a lying-in woman where there was suppuration present in the joints, high temperature, with suppression of the lochia, terminated by death. This form was well known to be that variety which was most likely to be conveyed by the midwife or practitioner to another patient. The origin of these cases was obscure. In many there was no discoverable lesion of the parturient canal, and yet, within a few hours, the symptoms above-mentioned would present themselves. When a patient in the Lying-in Hospital caught scarlatina or diphtheria, that was not puerperal fever, but scarlatina or diphtheria. He thought the most convenient classification was that introduced by Dr. Robert Barnes, viz., autogenetic and heterogenetic. It was difficult to treat puerperal fever. The wisest thing was to adopt prophylactic measures. These he had used for several years in the British Lying-in Hospital. No patient was left lingering in labor until her pulse and temperature had risen. When the os uteri was fully dilated, and the head made no progress, the forceps was indicated. Carbolic washings of the genitals, delivery, in the hospitals, under the carbolic spray, and due contraction of the uterus, secured by quinine, opium, and ergot, constituted the chief points in the immediate antiseptic system. Since he had adopted these measures in the British Lying-in Hospital, 450 women had been delivered, with only two deaths. This was, of course, far below the general mortality of lying-in women throughout the country. This was, at least, 1 in 100.

Dr. Alderson (Hammersmith) took great interest in the subject of metria. Cases of puerperal fever had caused him much anxiety. He thought it a matter of importance to recognize the disease at the earliest possible period; perhaps only then could fatal results be prevented. Too often, it was thought, puerperal fever did not occur after easy labors. His most serious cases had been after perfectly natural and easy labors; and it was, therefore, most important to be watchful after the easiest labor. Where metria occurred, there was, generally, at an early period, on the second or third day, some departure from normal convalescence; either a rigor, a quick running pulse, a distended abdomen, offensive lochia, diarrhœa, or excessive sickness, in acute puerperal fever always. He did not believe in serious cases of metria occurring in patients who were doing splendidly well up to the third or fourth day, as Dr. Lister mentioned in his remarks, recorded in the *American Obstetric Journal.* As to its origin, there were perhaps autogenetic causes, but they were rare; heterogenetic origin was the rule. The poison was brought from without. The result of his experience and observation was that, as Dr. Graily Hewitt had observed: "Puerperal fever consists, more or less, in an injection into the blood of some poisonous animal matter; it is a form of pyæmia." He regretted to say that he conveyed it, in attending three patients, twelve years ago, after examining and handling a case of erysi-

pelas of the scrotum, sloughing; although he had in the interval driven in a dog-cart to Hanwell, and some hours had elapsed. The first and third cases were both primiparæ; the second a multipara, and, in consequence, he believed, less liable to receive the poison, and had only some mild symptoms, and quickly recovered. The other two were fatal; they resembled each other in every particular. It might frequently follow attending patients after examining cases that would at first perhaps hardly be thought likely to produce metria, such as whitlow, small abscesses, etc. By being on the watch for the earliest symptoms of this dire disease, he had not had a fatal case of puerperal fever for several years; and also, by taking all prophylactic precautions against any chance of conveying any germs from septicæmic cases. It is an interesting and important question, when we must suspend practice. He had carried out for many years Dr. Williams' suggestion, at the Obstetrical Society, 1874, of disinfecting his clothes and himself with the fumes of iodine, and with the happiest results.

Mr. J. E. Burton (Liverpool) would pass over the differential diagnosis between two forms of metria, and glance for one moment at the cause of sapræmia, and some of the earlier stages of the disease. He would suppose a clear and undoubted case of sapræmia or putrid intoxication. The patient was poisoned by her own putrid discharges. But a patient's discharges should not be putrid, and would not be so without cause. This cause lay in too long retention of the normal muco-sero-sanguineous discharges. This abnormal retention of discharges was in its turn the result of imperfect *post partum* uterine contraction, or even of complete absence of them; and the matter was traced a stage further back. This want of contraction, or imperfect contraction, was the result of inhibitory influence, produced by chill, excitement, grief, or perhaps other kinds of nervous shock. Thus he brought back all these cases of sapræmia to imperfect *post partum* uterine contraction as their prime cause. In nearly all these cases, if a digital examination were made, the os uteri would be found gaping, the body of the uterus forming an actual cavity, the blind end of which lay at a lower level than the opening, so that the discharge could not escape unless the uterus contracted and expelled them. The uterine wall would be found hard, œdematous, and tender with its power of contraction almost or altogether in abeyance, so that the discharges which the uterus could not expel, and which could escape by their own gravity, from the position of the patient, formed a sort of pool, only the overflow of which found its way out. Under such circumstances, it was not wonderful that chemical changes set in. The germs of change were not far to seek; the intestinal canal was full of them, and it was no difficult matter for them to find their way through the intestinal walls into the vagina, in spite of antiseptic precautions and occlusive dressings over the vulva. Even in those cases in which por-

tions of decidua or placenta remained in the uterus, the course of events was much the same; the one point of difference being that, in the one case, the uterus failed to expel normal liquid discharges, and in the other the uterus failed to expel a solid normal discharge. In both, the first step in treatment was the timely removal from the uterus and from the body of everything that should be expelled. With this object in view, injections were employed to wash out the vagina and uterus; enemata of castor-oil and turpentine were used, and hot poultices were applied over the abdomen, as both these means produced uterine contractions; and further, with the same object in view, he had for some time past always administered ergot, and kneaded the uterus as far as the case would allow. He considered these latter important adjuncts to the other treatment. It was about time to cast off the traditional binder, at any rate after the first twenty-four hours; and, in place of it, to resort to inunctions, kneading, and massage. If this were adopted as the routine practice, the uterus would be stimulated to more effectual contractions; there would be less sapræmia. It ought to be the first duty of the medical men to act the part of police officers, and see that all discharges "moved on." It was better to get rid of them than to try to render them innocuous. He had intended to speak of the connection of erysipelas with metria, and would mention that the genital parts ought always to be carefully scrutinized in every case of pyrexia, for the purpose of ascertaining whether any lacerations had become erysipelatous, and to observe that he saw a case not long ago in consultation that ultimately recovered. The gentleman in attendance, he afterwards learned, had, within a short period, had five cases of metria; and an inmate his own house was at the time suffering from erysipelas. With regard to other infectious diseases, he thought he had seen typhoid fever take on the symptoms of metria, both with a favorable and with an unfavorable termination. Scarlatina he had seen both with the distinctive rash and without it. When the rash was absent, the scarlatina was most likely in an abortive form, i.e., the patient had already had the disease once, and was incapable of developing it in its perfect form a second time. When scarlatina broke out, in a house, those who had already had it might suffer from pyrexia and sore-throat of short duration; and he held that such a scarlatinal pyrexia, attacking a lying-in-woman who had already had the disease, might in place of it set up scarlatinal metria. He thought he was supported in this by the frequency with which, in times past, the introduction of scarlet fever into lying-in hospitals was followed by an outbreak of puerperal fever. On May 16th, 1875, a patient died of scarlatina in the Rotunda Hospital, Dublin. She probably suffered from the disease a few days before she died. On the 17th, two deaths were reported from peritonitis, or, as would be said, "metria." There was another on the 20th, one on the 23rd, another on the 24th, another on the 25th, another on the 27th,

a nother on the 28th; another on June 3rd, another on the 15th, another on the 29th; one on July 14th, and one on the 15th. Thus a death from scarlatina in a lying-in hospital, in which infection could easily spread, was followed by thirteen deaths from metria within a short time. Of course, there was no absolute proof that the scarlatina was the originator of the succeeding cases of metria, but the probability was strongly in favor of such a belief being well founded.

Mr. Donovan (Witwick) thought the name of the disease called puerperal fever was of little moment. He had had a good many cases, and had generally found that it arose not in cases of difficult labor, but in those which had no difficulty. He had for several years—since reading Dr. C. Bill's paper on the treatment of puerperal fever—given frequent and large doses of tincture of muriate of iron (*Edin. Phar.*), having first cleared out the bowels. This treatment he had never found to fail. Some held that the tincture of the muriate of the *Edinburgh Pharmacopœia* and the tincture of the perchloride of the *British Pharmacopœia* were the same. This was wrong. He believed that the injection of carbolic and other antiseptic solutions were thoroughly wrong, and most injurious. Before the uterus could be washed out, the fluid must be introduced into it, and, with open vessels and a patulous os uteri, there probably would be absorption of some of that fluid. He did not see the use of introducing a noxious fluid into an organ which was in the most favorable condition to absorb that fluid. The after-pains were nature's means of clearing the uterus of septic fluids; and he thought the early getting out of bed and the giving up the use of the bed-pan would do much to help the uterus in clearing itself of the retained fluid, which might probably help to set up septicæmia. The common idea that puerperal fever must be usually fatal he thought a grave mistake. He could not see any reason why puerperal fever should not, under proper treatment, become a disease which should possess few terrors for the patient or for the practitioner.

Dr. Grigg (London) asked if there were any such specific disease as puerperal fever; or did the disease embrace many affections altered or modified by the peculiar state of the blood and nervous system of the puerperal woman? He inclined to the belief that the reason there was such confusion of thought and such difference in regard to this disease, arose from the fact that few, if any, called upon to attend puerperal fever, had an opportunity of seeing a post mortem examination of the woman attended. The question could only be decided by carefully conducted post mortem examinations. The results obtained by an examination of such cases were as follows.

1. The disease began in the uterus. These were cases where operative measures, manual or otherwise, had been adopted; or where there were protracted labors, infection by attendant, or endemic influences.

2. The disease took its origin in the pelvis. These cases were long standing or recent pelvic disease, or cæcal mischief; the uterus being intact.

3. There was general or diffused peritonitis with no uterine mischief. These cases were long standing or recent disease of the large internal glands, kidneys, liver, etc. The result of these examinations was that erysipelas appeared to be a form of disease, if not the disease itself. He had never seen an epidemic of puerperal fever, where local erysipelas or phlebitis had not occurred at the same time. Again, the constitutional symptoms were exactly analogous to those of erysipelas. He believed the disease to readily respond to proper management. The disease, if left to itself, terminated in a crisis by vomiting or purging, or both. If much opium were given early, it arrested the peristaltic action of the bowels, which could never again be restored. An impression should be made on the system by the administration of sudorifics coupled with opium; either Dover's powder, or the hypodermic injection of morphia. Iron should be given, not too early, but after the second or third day of the pyrexia. Quinine was a most useful remedy, but it was not right to administer it too early in the disease. It was much more effective if given later on. A good emetic in the form of ipecacuanha was an excellent remedy, if given early. Injections should only be used when there was an offensive discharge.

Dr. Alexander (Liverpool) said there were two kinds of so-called puerperal fever; the first where the disease was in reality erysipelas; typhus or scarlet fever. In such cases, the epidemic disease modified the conditions of the lochia, and produced a metria that, in its turn, modified the epidemic disease. In the other class of cases, the disease always began in the uterus, and was really a septicæmia dependent on the altered conditions of the uterine contents, produced by obstruction to the flow of lochial fluid, nervous conditions that relaxed the uterus, putrid poison introduced from without, etc. The putrid discharge was absorbed, and poisoned the patient. Acting upon this theory, his treatment has been, whenever the fever was high, to make the uterus contract by pressure of the hand on the abdomen, and put on a firm binder. He gave a dose of ergot and liquor ammoniæ acetatis, continued every four hours. If the patient's temperature did not soon abate, i.e., in a few hours, he washed out the uterus carefully once, and continued the former treatment. Since adopting this treatment, he had had no trouble with puerperal fever. The treatment must be adopted early.

Dr. Wynn Williams (London) considered those cases only as puerperal that where due to septicæmia, which might rise either from within or without the body; when arising from without, there must be suppuration, such as there was in scarlet fever, erysipelas, etc. It generally,

however, arose from within, due to the retention of clot or other animal matter becoming putrid. The object, then was to destroy the septic matter, which was best done by syringing the uterus with tincture of iodine, three drachms to eight ounces of water, and continuing the process until the fluid returned to the same color as before. Experiments on guinea-pigs had satisfied him that iodine and septic poison could not exist together. A practitioner might surely free himself from all trace of septic poison by placing some grains of iodine in a saucer, and applying a spirit lamp to the bottom of it, and allowing the fumes to fall all over his person.

Dr. A. D. Macdonald (Liverpool) was happy to support the view of Dr. Wynn Williams that washing the hands with a weak solution of iodine was a great means of prophylaxis. He had recently attended cases of erysipelas and of scarlatina—using this solution—and having no metria following. A short time ago, he was called to a case of puerperal fever delivered by a midwife where she, after warming and using the iodine, as well as having a short holiday, had not had a bad case. This case illustrated the communication of the contagium through hand-shaking with a nurse who had attended a case of septicæmia.

Dr. Edis (London) thought the whole question of metria was of so much importance to the practitioner, that anything throwing light upon it was of interest. Prevention was the key-note: not to allow the patient to drift into powerless labor; to secure efficient contraction of the uterus and expulsion of all the decidua; to be scrupulously cleanly in all the appointments of the lying-in room and in those in attendance there; to avoid all risk of infecting the patient with any contagion, whether exanthematous or otherwise. If febrile symptoms occurred, the proper course was, to wash out the uterus, to sustain the patient's powers by appropriate nourishment, and stimulant if necessary, and to encourage rather than check the natural tendency to vomiting or diarrhœa. Quinine and opium should be given, or iron, as might be indicated. The mere name of puerperal fever was merely a comprehensive term to express many and varied conditions occurring in the lying-in patient, and not any specific disease.

Dr. Nesfield (Manchester) thought a large number of cases of puerperal fever were due to septic poison, and arose in the uterus itself from membranes, clots, or lochial discharge. Still, many cases arose from poison outside, as erysipelas, or scarlet fever, and especially the malignant forms of scarlet fever. The treatment of these two classes of cases must, of course, be very different in its results. When the disease was autogenetic, the uterus might be cleansed by washing it out with a double-stream catheter; and perhaps the poison might be quickly got rid of. It was not so with an erysipelatous poison; still here the syringing of the uterus with a mild antiseptic lotion was useful. His plan was, if there were any deviation in the ordinary convalescence, to take the temperature; and, if there

were any serious increase, to wash out the vagina, and perhaps the uterus itself. As to the contagiousness of puerperal fever, if sporadic and due to auto-infection, simple precautions by means of disinfectants were sufficient; but if there were erysipelas or scarlatina, much evil must follow any further attendance on cases of midwifery.

Dr. Wallace (Liverpool) pointed out some of the principles which ought to guide in considering this disease. The term was misleading, or at least confusing. It would be better to diagnose the fever proper which attacked the patient; and Dr. Wallace could only recollect one case where the medical man had carried scarlet fever to the house and children of the patient, and to the patient herself, from which she died. Contagious exanthemata were rarely carried by the medical man; but, on the other hand, septicæmia had far too often been carried by hospital-surgeons to patients. The point to be noted from this is to keep the hands septically clean; and the clothing ought always to be changed, if the practitioner has been attending a case of contagious disease. With these precautions metria might be checked, and, except in autogenetic forms, prevented.

Dr. Graily Hewitt (President) congratulated the Section on the valuable information elicited from the various speakers. As to the term, he agreed with Dr. Thorburn in advising the use of metria as a designation for the disease. Although the speakers had expressed opinions of a different character, there was a general concurrence on certain important general principles, and that this serious disease must be considered as not only preventable, but in most instances curable. The prevention of the disease was secured, first, by prevention of the introduction of septic matter from without. The autogenetic cases were, he considered, common; and in this class of cases the important point was to raise the health and strength of the patient, and thus prevent absorption by ensuring contraction of the uterus. Mr. Burton's and Dr. Alexander's observations showed the great importance of uterine contraction in curing the malady. On this subject, he was pleased to find the treatment found so successful by Dr. Alexander was identical with that recommended by himself at the discussion on puerperal fever at the Obstetrical Society of London some years ago. He mentioned a case of mental shock producing severe metria, cured by pressure, copious administration of food, and stimulants. Here the pressure was found sufficient without intra-uterine injection.

Mr. Wilson (Liverpool) said that the medical man in attendance on a woman in labor should never make a vaginal examination without previously thoroughly washing his hands, using the nail-brush and disinfecting soap, if at hand. The obstetric bag should always contain a pot of vaseline, to which carbolic acid or thymol had been added.—*British Medical Journal.*

Abstracts and Selections.

I.—OBSTETRIC.

Fatal Arrest of the Digestive Process from Chloroform, Following a Difficult Labor.—Alexander Hutchins, A. M., M. D., contributes the following to the *Proceedings of the Medical Society of the County of Kings:*

Mrs. B., age 24, primipara; health excellent throughout pregnancy; all physical functions normal; appetite and digestion good; able to attend to all needful household duty and to take all desired exercise out of doors; moral condition cheerful throughout; social surroundings easy; complexion brunette; stature average; habit full; physique normal; no albumen in urine throughout pregnancy.

At 2 a.m., May 9th, 1883, was awakened from sleep by a profuse, drenching discharge of liquor amnii without pain. The discharge ceasing, and no occurrence of pain, went to sleep again, and the writer was not notified till 9 a.m. Patient had breakfasted, and was dressed. No dilatation of os and no discharge; soft parts normal. The patient being at expected term, was seen occasionally, but with the exception of slight pains, without dilatation, at intervals varying from one to three hours, no change occurred in her condition till the afternoon of the 11th.

At 2 p.m. of the 11th, just sixty hours from the discharge of waters, uterine action became regular, with the first signs of dilatation. The pains recurred at intervals of about fifteen minutes from this time till 6 p.m., then became quite severe at intervals of about five minutes, with so slow dilatation, that at 10 p.m., the os was not larger than a silver dollar. There was no further discharge of liquor amnii, the soft parts retained their naturul secretion and temperature, but did not soften as in ordinary labor. The patient was cheerful and bright in the intervals of pain. The os being dilatable, and the labor so long continued and promising so great tediousness, when the attendant was considering the propriety of interfering to facilitate delivery, at about 10:15 p.m., at the onset of a pain apparently no more severe than its predecessors, a convulsion occurred.

Dr. J. B. Leary responded promptly to the call for assistance, but, pending his arrival, the forceps were applied and without difficulty. The full effect of chloroform being secured, traction was begun without apparent result. Dr. Leary and the writer relieved each other in the effort at delivery for about an hour, the anæsthesia being steadily sustained, as any traction made during any stage of consciousness, produced the most violent resistance. Dr. J. J. Lamadrid was then summoned and the three took turns in traction and administering the chloroform.

After about two and one-half hours of this effort the Bedford forceps failed to secure the head, which, in its slow progress through the unyielding canal, became cylindrical in shape, though it had been forced down to the symphysis, a portion being locked on the pubes and refusing to be brought down to the perineum. Matters were at a standstill and it seemed as if craniotomy was inevitable. The Hodge forceps were then applied with the traction directly at right angles to the axis of the body. After about twenty minutes this manœuvre proved successful, and the head was born. Dr. J. H. Hunt arriving at this juncture, after a prolonged aud tedious effort, succeeded in liberating the arms, and after some further and unusual effort the body of a nine pound child, dead. The chloroform was withdrawn, the placenta extracted from the uterus, which contracted promptly, a hypodermic injection of ergot administered and in the course of one hour the patient was left apparently thoroughly comfortable. The laceration was inconsiderable and to one side, the sphincter ani not being included.

At 9 a.m. of the 12th the patient was cheerful and free from serious discomfort, had slept for five hours, passed water voluntarily, and had had an action of the bowels. The lochial discharge was natural and very slight swelling of the vulva, with no unusual soreness.

By noon of that day a nausea, which had caused some uneasiness during the forenoon, became quite distressing, efforts at vomiting were frequent, and eructations of gas abundant. Later in the afternoon colonic distension was evident and the tympanites caused grievous distress. Volumes of gas were discharged per anum, explosively and with great frequency, oftentimes accompanied by natural fæces. Food of whatever kind, taken into the stomach, was followed by this enormous secretion of gas, and when the food was withheld there was no appreciable difference in the amount of distension. Innumerable devices, both of food and medicine, were resorted to as a relief to this indigestion, but without avail. The suffering from this distention was very great and constituted the only distress she endured. Her intellect was clear, her apologies, over the explosions of gas and the accompanying discharges of feces, were unfailing. She had control of her urine, and passed it in sufficient quantities, it being usually light in color. At no time was there any approach to delirium. Her self-control was surprising. She made little complaint, save as the pain from movements of the gas in the bowels, or some change in her position caused an involuntary shriek. The frequent and voluminous explosions of gas per anum caused on the second day paralysis of the sphincter ani. She made strong efforts to obtain sleep, but the frequent paroxysms of pain above described prevented sleep, except when it was forced by the partial narcosis of an opiate.

The aid of Dr. J. H. H. Burge was invoked during this period, but all means proved futile to modify or arrest the trouble.

This condition of affairs, beginning in the forenoon of the 11th, a few hours after delivery, continued, without material change of history, till about 10 a.m. of the 17th, when after the morning toilet had been made with considerable personal assistance which she was able to render, after having had some food, with a manifest clearness of vision and intellect and with no apparent change in her condition from what it had been for six days, suddenly, and with one of the sharp twists of pain a convulsion ensued, and in a short time she was dead. This very distressing experience presented some extraordinary features, ante- and post-partum.

The labor was markedly a "dry labor." The waters were discharged sixty hours before dilatation began, and no leakage occurred in the mean time. The child was strongly compressed by the uterus during this period. The dilatation was slow and painful and the ordinary relaxation of the soft parts did not occur. The passage of the child from the uterus to the external world was through an unyielding canal, or unyielding to the extent that all progress was made by forcing a slowly compressible large body through a slowly dilating small canal, and this through its whole extent, the head of the child becoming more and more cylindrical till its delivery was completed. The distortion of the head prevented its passage into the hollow of the sacrum and caused it to impinge against the pubes, requiring for the final manœuvre in its delivery, an unusual mode of traction with the exercise of great force. The extreme pressure of the advancing child provoked the most violent reflex resistance, which only profound anesthesia could overcome. The first convulsion was the result of pressure and delivery without chloroform would have been impossible. The anesthesia was continued for three hours and was well borne. The recovery from the prolonged anesthesia was rapid, with immediate and full consciousness. Though the writer is compelled to include himself in the laudation, that due credit may be given to the gentlemen who so promptly, laboriously and skillfully aided him, it is a tribute to the skill and wisdom of the physicians in charge that such tremendous and long continued efforts at traction, in such difficult straits, were conducted to a successful issue with so little laceration, that, under ordinary circumstances, it would have been of no concern. The immediate post-partum conditions were such as belong to the most ordinary labor. The urine and feces passed naturally, the uterus normally contracted and the lochia was of typical quantity and free from any septic condition. Neither the temperature nor surface indicated any febrile action, the eyes remained clear and the tongue gave very little evidence of gastric disorder. No milk was secreted. The intelligence was unclouded, and the moral sense in repose. In fine, the whole parturient epoch, saving the early discharge of the waters and the extraordinary difficulty in the actual passage of the child, included no elements different from the experience of an ordinary labor.

The sudden arrest of the digestive process, that followed so speedily after the delivery, has been portrayed above, and this, with its consequent complications, led to the fatal issue. It is the opinion of all who were familiar with the case, that the digestive disturbance was directly due to the secondary effects of the chloroform. Delivery without the chloroform would have been impossible. The patient would have died from shock, had the attempt been persevered in, long before delivery could have been affected. In fact, the complications were so grave that not one of the physicians or attendants expected that she could survive the birth of the child.

Her earliest complaint, the morning after the labor, was distress at the precordia, as if there were a load that would not be removed—a dyspeptic gastralgia. She spoke often of the continuing odor and taste of the chloroform. Her attendants perceived it constantly for a couple of days. She was saturated with the anesthetic. The long period during which she was under its influence, aggravated to what proved a fatal degree the digestive disturbance, which, in varying shades, affect all who come out from a profound anesthesia. The writer has frequently noticed the disgust for chloroform that is excited in those who have used it for recurring neuralgic conditions. Many prefer to endure the pain rather than the after effects of the anesthetic. In this case the arrest of digestion was so complete that none of the multiform devices resorted to availed to restore the natural process. As reported above, a normal dircharge of feces occurred within a few hours after delivery. The subsequent movements were less thoroughly fecal, and after the bowels had discharged their contents at the time of delivery, no further fecal discharges appeared, showing that the digestive process never re-asserted itself. The enormous distension, which was rarely modified even by the voluminous expulsion of gas, was a bar to all normal digestion. All remedial measures, as well as all efforts at nourishment, were foiled by the incapacity of the organs of digestion to appropriate them. The convulsion that closed this protracted suffering was a counterpart of the convulsion that ushered in the full dilatation. In both cases they were reflex, in the latter from the pain of pressure, in the former from the pain of distension.

So far as is known this case is unique as illustrative of the possible profound effects of prolonged chloroformization on the digestive function. This patient, who had been in perfect health, with a sound digestion all through her pregnancy and had gratified her appetite with hearty food, had taken her usual food during the sixty hours that intervened between the discharge of the liquor amii and the onset of labor, and had taken some light food during the ten hours that preceded the convulsion, so that the usual rule of taking chloroform on an empty stomach was not, and could not have been, adhered to in her case. It is not unlikely that the digestive process had been held in abeyance during the period

of dilatation, and that vomiting of the contents of the stomach did not occur by reason of the prolonged full anesthesia, and thus the conditions, most fertile for the development of the indigestion existed.

If this be true, serious and distressing as this history proved, it has little lesson for the future save as it amplifies and enforces the rule of dieting in advance of the anesthetic. The vomiting that might have occurred in an interval of partial consciousness, did not take place, and a greater or less quantity of undigested food lay, as an irritant, in the paralyzed digestive track. This line of reasoning, recognized and acted upon at the outset, led to the administration of a mild aperient within the first few hours and as the discharges during the second day showed the absence of any obstruction, the indigestion was maintained to the end by the muscular paralysis consequent on the continued distension.

Anœsthesia in Obstetrics.—By Dr. Just. Lucas Championniere. (Translated by D. C. Holliday, M.D).

The administration of chloroform to women in labor is one of the most interesting subjects to obstetricians in general practice.

It is a well-established fact that if the majority of our confreres are obliged to allow a large number of their female patients to go through labor without assistance, there is a certain number where the use of chloroform could be easy, and others when its use becomes a necessity.

It must be admitted that we are at present without a recognized and satisfactory guide on this important point.

Many trifling publications have appeared. Campbell published the first part of an interesting pamphlet on this subject, but, unfortunately, death prevented its completion.

A number of articles have appeared from time to time; some good ones among them we might mention, especially an excellent thesis by one of our former pupils, Dr. Despian, entitled "*Etude Clinique do Chloroform dans ees Accouchements Naturels*, 1879."

The question, however, is somewhat neglected, and we hail with pleasure the recent publication of an excellent thesis of 350 pages on this interesting subject by one of the distinguished pupils, of our Parisian hospitals, Dr. Dutertre, entitled "*de l' Emploi du Chloroform dans les Accouchements Naturels.*

This work treats fully of the history, the physiology, and all details of the use of anæsthetics in obstetrics.

The author, with singular tact, has purposely and wisely avoided too extended clinical discussion for the excellent reason of his own youth; nevertheless, this volume contains all that is necessary for any one to make an exhaustive study of the subject. Everything is clearly expressed, with great originality in style, which in nowise detracts from its interest.

M. Dutetre, having carefully studied the physiology of his subject, clearly demonstrates the great necessity of the use of pure chloroform, an idea which we ourselves have recently called special attention to.

The perusal of this work brings forcibly to our minds the similarity of our teachings on this subject, and we cannot but think that our readers will thank us for the further discussion of those practical and clinical facts bearing on this subject without a repetition of what we have already published.

Those authors who deny the practicability and usefulness of producing in a woman during labor a semi-anæsthetic condition compatible with consciousness, have done so without sufficient practical experience.

All discussion falls to the ground when disproven by extensive clinical trial and experiment; such has been my course not only in private practice but in the wards of one of the most extensive hospitals in France.

The method of using anæsthetics must differ essentially from that usually employed during ordinary surgical operations. In exceptional cases the inhalation must be slow and progressive.

Ordinarily from ten to twenty minutes is required to produce calmness, and a semi-anæsthetic condition; this is a result extremely difficult, not to say impossible to obtain, unless the patient is docile and willing.

Some women require but an extremely small quantity of the anæsthetic —twenty to thirty grammes for several hours. However, should the condition of semi-anæsthesia be required to be kept up for many more hours, from 16 to 100 grammes will be necessary.

When I order chloroform I usually divide it into two vials, 50 grammes each, and I rarely find it necessary to employ more than one vial, unless operative interference is required, and it becomes essential to make anæsthesia complete.

For a long time I taught that some women required much larger quantities of chloroform to produce the same effects, say 100, 150 to 200 grammes. But since I paid attention to the quality of the chloroform used, now only prescribing the purest to be obtained, I find these differences far less marked.

Indeed, the attempt to produce semi-anesthesia by the use of ordinary chloroform is to court failure.

There are some women in whom, without producing muscular relaxation, it is necessary to push the anesthesic to a partial loss of consciousness before perfect calm is reached.

In case of necessity this is productive of no ill effects. I have myself maintained a state of complete anesthetic insensibility, or coma, varying from three to seven hours, without danger.

Among other cases I will cite one, where I performed Porro's operation, on a woman who had already been fully under the influence of chloroform for three hours; the operation required one full hour for its

completion, and the patient recovered without a bad symptom. Here, there was the absorption of 370 grammes, of chloroform.

I merely mention this to show that large quantities are often tolerated. I have already stated that the period of dilatation of the uterus was frequently appreciably shortened by the early use of chloroform; hence my rule is never to wait for the expulsive stage only, for commencing its use.

No definite time can be fixed for the continuance of anesthesia; this necessarilly varies in different cases. Two or three hours are usually sufficient; eight or nine hours or more, may become necessary.

In many of those cases requiring a prolonged use of the anesthetic I have carefully followed up their subsequent history, and in no single instance have I been able to discover any injury either to mother or child.

Is the use of chloroform during labor absolutely without danger? This may be positively affirmed, if used in the manner we have pointed out.

Does this mean that no precautions are necessary in its use? Most certainly not. It is not, however, because death is at all to be dreaded in its use.

M. Dutertre in his excellent thesis shows with what eagerness the opponents to the use of chloroform, detail facts and observations in support of their position, which upon careful examination are proved to be fallacious, and often even ridiculous.

Those, however, who make a daily use of chloroform recognize that they often meet with threatened accidents which should never be neglected, showing individual idiosyncrasies forbidding its use.

No one more than myself has insisted upon these peculiarities, and our readers are already fully aware of my views, elsewhere expressed.

According to their peculiar willingness to adopt or reject the method of using chloroform advised, some authors recognize more or less cogent reasons for its indication.

It may be stated briefly, that pain is the only true indication; where pain is really severe, or where it is really unbearable; whenever pain is excessive, no matter at what stage of labor, recourse may be had to chloroform.

Its early employment undoubtedly accelerates, rather than retards, the progress of labor, for the excellent reason that the uterine contractions subsequent to the use of the anesthetic, soon revive after a period of marked relief, and continue with more regularity and force.

A certain degree of rigidity of the cervix readily yields to the effect of chloroform, but this happy effect cannot always be expected if this rigidity had been allowed to continue for many hours. When a woman in labor has suffered intense pains for a long time it becomes the duty of the medical attendant to administer chloroform. Often in these cases

the necessity for immediate effect renders it imperative to push the chloroform to perfect anesthesia, for it is a recognized fact that in a woman thoroughly enervated by long suffering it is much more difficult to obtain the satisfactory effect of chloroform.

The contra-indications to the use of chloroform have been detailed by many authors with such fanciful precision as to show that they are not the result of experience.

Thus a cardiac affection is considered a contra-indication to its employment.

It has been my good fortune to have had many excellent opportunities to prove the contrary, and I shall proceed to mention one among many cases.

The subject was a lady affected with a congenital affection of the heart, when, by the judicious use of chloral, pregnancy had been prolonged to full term, after a series of previous miscarriages. Thanks to the early use of chloroform every stage of labor was accomplished with ease and rapidly notwithstanding a rather stormy beginning, and by the application of the forceps at the inferior strait she was delivered of a child, weighing upwards of 4 kilogrammes.

It is very certain that to relieve fruitless effort, and prevent continued muscular contractions, is a positive benefit to the heart.

A dense pulmonary affection may be a contra-indication; here the physician's responsibility becomes much greater.

Pulmonary lesions may be rendered threatening, if the anesthetic be pushed too far. Nevertheless, I have frequently used chloroform freely in different stages of phthisis, and once even during the course of pneumonia with the best possible result both for mother and child.

Are any accidents to be feared? I have mentioned phenomena of apnœa, many of which are undoubtedly attributable to the bad quality of the chloroform.

However, I have no doubt that rare and exceptional cases may be met with.

Inciting the thorax and insisting upon free respiratory efforts, on the part of the patient, are followed by their rapid disappearance.

Can vomiting be the result of its protracted use? This is not absolutely impossible, yet I have never seen it; but I have frequently seen chloroform put a stop to obstinate vomiting during labor.

Possibly the most important objection made against the use of chloroform is that it favors the occurrence of hemorrhages.

A careful examination of the testimony of various authors proves that their opinions are simple allegations rather than established facts.

A careful review of our own observations does not at all corroborate the truth of these allegations.

On the contrary, if we did not hesitate to assert facts without proofs,

we would be inclined to believe that chloroform rather prevents the repetition of useless uterine contractions, and puts the patient in better condition for permanent uterine contraction. This appears to be the result in a number of cases.

The same may be said with regard to puerperal condition. Not only has it been equally satisfactory, but the return to health has often been more easy and rapid.

Another theoretic objection has been raised, claiming the deleterious effect of chloroform on the infant. First, the patient rarely takes much chloroform; however, admitting that she took a great deal, I have never, in a single instance, noted any injurious effects on the child. A fact too, worthy of remembrance, is that a new-born infant bears chloroform very well.

It is probable that anesthesia in obstetrics might be produced by the greater number of recognized anesthetics. I, myself, have only tried ether, and the bromide of ethyl.

Both have appeared to me inferior in action to chloroform. Ether, without a special apparatus, fills the room with dangerous vapor, its action, too, is too slow. Bromide of ethyl emits a nauseous vapor, very disagreeable even to the accoucheur himself; its action is much slower, and not by any means less dangerous than chloroform; on the contrary, notwithstanding the hopes entertained of its many advantages, by its early advocates, the records of its use in general surgery has been that of frequently producing most formidable accidents.

In general practice, it should be borne in mind, that chloroform used to produce partial anesthesia. and pushed to complete anesthesia where occasion requires it, is of such great use that the accoucheur should never be without it. The chloroform used should always be chemically pure.

At the same time that I always have pure chloroform at my disposal, I also provide myself with reliable ergotine—whether we simply use it hypodermically in case of hemorrhage, or where you make a systematic use of it as I do, after all deliveries.

After complete anesthesia, the use of ergotine is far more reliable than any preparation of ergot.—*N. O. Med. and Surg. Journal.*

Report on Gynecology and Obstetrics.—By Geo. J. Engelmann, M.D., Chairman of Committee, to the Missouri Medical Association, at their meeting at Jefferson City, May, 1883.

By the coutesty of our President I have been appointed chairman of the Committee on Gynecology. I believe that it is expected from the chairman of any committee to offer a brief retrospect of the progress which has been achieved in his respective branch during the past year. To the busy practitioner this is an almost impossible task, rendered espe-

cially so by the vast amount of work everywhere done by busy workers in this important branch, I may say, of medicine and surgery. New operations, we have none to record since the last meeting of this society. Some are now being tested, such as Porro's operation and Thomas', but these, performed in rare instances only, are hardly of practical interest to us. The features of leading interest in operative gynecology have been mainly the steady progress in the success and beneficial results of surgicial interference · the question of the use of Listerism in abdominal section; the gradual development of Battey's operation which is now finding its proper sphere as extirpation of the ovaries, widely different from the one first suggested by the originator; and what is perhaps of the greatest practical interest, the gradual advancement of Emmet's operation or trachelorrhaphy into its proper channel.

In the busy work everywhere done, individual operators have individualized their work. Modifications of operations, of instruments, of methods of treatment, have been advised, each deemed to be especially serviceable and especially successful by the one especially skilled in the one manipulation, in the use of the instrument, which is of minor importance, and I would here refer only and that briefly to the points last mentioned: the use of antiseptics in gynecological operations, and operation for laceration of the cervix.

In speaking of the use of antiseptics in gynecological operations, I would make a distinction between operation per vaginam, or rather externally upon the vaginal tract, and abdominal section intra-peritoneal. With regard to the former class of operations I cannot but say that the results of a thoroughly antiseptic method have proven in my practice, at least, almost astonishing, and my results have been throughout such that I cannot but advocate the method most earnestly. It is a thoroughly antiseptic operation. The cleansing of the parts before operating; the use of the hot douche of carbolized water during the operation; the hot douche, both as an hemostatic and as a cleanser to free the parts of the septic matter and do away with the necessity of sponges, with carbolic acid added as a disinfectant. Carbolizing the silk which I now use altogether for sutures, carbolizing also the wound; dressing with iodoform, not sparingly used but in quantity; when the second dressing will only be found necessary after removing the sutures; when open, cleansing with carbolized water. The iodoform dressing is probably all that is necessary. For elytrorrhaphy, trachelorrhaphy, and whatever operations may be made upon the cervix or the body of the uterus, from within the vagina or the external genitals, this course will be found universally satisfactory. I might say, that not only there is no suppuration, but that often not even one single drop of pus is anywhere found. The union is always by first intention, and not even the stitches, if left in for ten or more days, cause the production of any visible particle of pus. Some little is found

about the sutures, but never more than a few drops; rarely any at all. Absolute cleanliness might possibly achieve the same result; but although this might prove satisfactory during the operation, the dressing to my mind must of necessity be thoroughly antiseptic, and of antiseptics I have found iodoform to be for this purpose preferable to all others. I cannot but most heartily advocate a thoroughly antiseptic method in all operations upon the general tract, but above all careful attention to the first dressing, when the second will rarely be found necessary until after the sutures are removed. With regard to abdominal section, the question seems an open one—the results of the ablest operators, some operating with antiseptics, others without, being equally good—the results of those operating without antiseptics being if anything better. This is a question between a few of the most skilled operators. Where the conditions are given for the observation of perfect cleanliness, surgically speaking, in all its details, antiseptics may not be necessary; but generally speaking, I would deem it dangerous to do away with this precaution. Provided the use of carbolic acid be not overdone and its dangers kept in view, it seems to me a matter of indifference what antiseptic is used. I have in two cases of abdominal section operated successfully with the use of Listerine for spray, dressing and sponges, and I believe carbolic acid may well be resorted to, provided it is used to a moderate extent. I fear that I lost my first case of ovariotomy from carbolic acid poisoning; and I certainly have seen ill effects in a number of instances —marked poisoning, simply from carbolic acid dressing to a small wound where only a few stitches had been applied; carbolic acid poisoning from vaginal injections. However valuable as an antiséptic, we must not forget its dangers.

EMMET.

The operation first advised by Emmet for the cure of a lacerated cervix is perhaps at present calling forth more general interest than any other. Loudly vaunted by some, depreciated by others; frequently performed here, avoided there; some claiming marvelous results, others only failures, it seems gradually drifting into its proper sphere. This operation, peculiarly American, still seems to be looked upon with distrust in England, but has been readily accepted by the operators of Germany, where, however, it is more frequently combined with some other surgical interference. Skeptics ridicule the gynecologists with the apparently puzzling question, why years ago every cervix was cut; why now every cervix is sewed. Both operations, while novel, before their proper sphere was found, were overdone; each operator hoping to achieve the brilliant results which followed the first operations, leading to their great popularity. In the early days of an operation enthusiasts are liable to see cases far beyond the proper limits, and in their enthusiasm report questionable results which spur others on, and failures are liable at that time to weigh

too heavily in the scale. The uterus, perhaps, especially the os and the cervix, are the seat of still unknown nerve endings whose communications are often most marvelous and equally uncertain. We know how a contracted os, an erosion, or a small polypus in the cervix, causes the most distressing vomiting, convulsions or reflex symptoms of other kinds, which will be at once relieved by the dilatation of the os, the cauterization of the erosion, or the removal of the polypus, and yet in many other cases we find a contracted os, granulations or polypi without reflex symptoms of any kind, with perfect good health of the patient. So it is with a lacerated os. The most remarkable reflex symptoms, the loss of mental and physical power may follow in one instance, whilst in another a widely gaping os is accidentally found in the examination of a patient, perhaps for pregnancy, in the best of health. Lacerations of the os are not necessarily followed by untoward symptoms. A small laceration may lead to distressing reflex symptoms, to mal-nutrition, to gradual sinking; whilst in another case a large laceration may be without any evil consequences whatsoever. Hence, we may safely say that every laceration should not be operated upon; nor should the operation be performed until we have attempted to relieve the old morbid conditions which exist, and certainly whether the laceration exists or no, these accompanying morbid conditions should be relieved. As it is unquestionably of all minor operations upon the uterus the one which is followed by the most striking and brilliant results, it is worth our while to look more closely into the indications and to study the conditions under which it should be performed. Out of some thirty cases, I have in two only met with a negative result, and in one of these it was, at least partially, due to the early departure of the patient and accompanying unfavorable circumstances. The other is unaccountable to me. The symptoms were such as I have been in the habit of seeing, nor could I detect any other disturbances, and supposing this to be the only source of the reflex symptoms, I operated and operated again, supposing that possibly the result might not have been quite as perfect as desired, but without any result. The negative results are certainly in some instances due to the accompanying endo-metritis, and it has now become my rule in these cases to curette the uterus thoroughly, and only after I have thoroughly cleared the uterine cavity of diseased products do I close the laceration. As some lacerations exist without causing any disturbance, and as the reflex disturbances, caused by the laceration are of the most kinds, it seems to me of the utmost importance to define the symptoms which come from the laceration and which might be indicative of operative interference. As there are but few local symptoms, the most important being of a reflex nature, it is difficult to determine definitely their character; but so many cases have passed under my observation, fortunately in a clientele whom I have been able to observe

and to follow after the operation, who have been able to follow instructions strictly and understandingly, and above all who have well depicted their symptoms, that I would not hesitate now upon receiving the history of a patient to diagnose the condition, if it was that of laceration. We will always find that the patient has lost both in body and in strength. She is growing more feeble; has different nervous pains in the back and sides, and she will always refer the beginning of her trouble within a time more or less limited after a confinement or miscarriage, and usually that confinement will be found to have been a very rapid or a tedious one, or to have been with a very large child; rarely if there was nothing unusual about the labor. Sometimes within the space of a month, sometimes three or four months, the patient begins to feel herself more nervous, the pains in the back and sides—usually the side of the laceration, or if bilateral upon the side in which the laceration is the largest—increase. These symptoms slowly develop. They are progressive, and as years go on the patient looses flesh. But in some cases where she does not lose flesh, she grows at least bodily weaker; her nerves weaken; she has rushes of blood; she grows excessively nervous; has backache, pains in the side, becomes indifferent to work, cares for nothing, and so she slowly loses ground whatever the treatment may be. Local applications; uterine treatment which has been resorted to, tonics, whatever is done, though it may help her for the time, does not cure; she steadily grows worse regardless of the treatment. I cannot enter into the details of the reflex symptoms, because they are of all kinds; in the stomach, breast, heart and chest, but as a class I think we may group these symptoms as they have been grouped, and I would add, that usually standing, walking up stairs, riding in the street cars, and riding on a rough road, causes an intense backache and aggravates all the symptoms. Differing in their details in every case, they harmonize thoroughly in the general outline above given; beginning after a rapid or difficult labor, or one where a large head has been forced through an unyielding os slowly or rapidly; the weakening of nerve and muscle; indifference and even mental weakness and steady aggravation of the symptoms, notwithstanding any local or general interference. Nothing but the closure of the laceration can relieve her, and this affords marvelous relief. When the operation is done without an anæsthetic, as I now usually do it, I have seen the patient leave the table with the expression: "Doctor I am a different woman." I have seen the backache, the pains, the nervous symptoms vanish in the course of a few days, but often certainly I have seen it take one and two months, the symptoms steadily disappearing, the patient losing her nervousness, appetite and strength increasing, and she certainly becomes in almost every instance a different woman, as I have again and again heard it expressed in those same words by various patients. Patients

who have been enfeebled; who have been so reduced that they have been confined to their rooms, almost their beds, for months and for years, have within a few weeks after the removal of the sutures, after the closure of the laceration, been so far improved as to be able to travel or to go about in pursuit of their duties. The local changes are equally marvelous. Accompanying the laceration is usually an enlarged, more or less prolapsed, retroverted, slightly retroflexed uterus and involution, more marked I might say than after labor takes place. Within a few weeks after the operation, even upon the tenth day, upon which I usually remove the sutures, I have found the uterus reduced in size, well up in the pelvic cavity and in proper position, and so it remains. In fact, I have been so confident of the rapid involution following the operation that I have in several cases, both in Vienna and Berlin, where the belief in the operation is not a very strong one, urged operators who were ready to amputate the cervix to desist from the operation and simply close the laceration.

So marvelous are in many cases the results of this operation, that judicious men have been led to doubt them, and but recently I was approached by one of our first physicians with the question of what I thought of this operation, which was now done so much. I can but say where other disorders of the uterus or its surroundings exist, these should be relieved, and if no marked improvement of the existing symptoms follows a brief treatment, the operation should be performed, although co-existing disorders should at the same time be remedied.

It has afforded me pleasure to have this opportunity of adding my mite toward the establishment of this most important operation in its proper sphere, and I trust it may not be long before we may be able to say that it has been truly defined.—*The Weekly Medical Review.*

Hot Water in Therapeutics.—Several years ago I learned in my personal experience that no agent relieves nausea and vomiting so satisfactorily and promptly as water as hot as can be drank. Since then, I have used it in a large number of cases, and it has been uniformly reliable. The following classification may be made of the cases in which it has been used.

1. Cases in which nausea and vomiting occurred at the onset or during the course of acute febrile disease.

2. Cases in which these symptoms were caused by overloading the stomach when its functions had been impaired by protracted disease.

3. Cases in which they were produced by nauseous medicines (not emetics) at the time they were taken.

4. Cases of acute gastritis caused by the ingestion of irritants.

5. Cases in which these symptoms were purely reflex.

6. Cases of chronic gastritis.

7. Case of colic in newly-born infants.
8. Cases of flatulent distention of the stomach.

Amomg the cases of class 1 was a case of diphtheria and one of puerperal septicæmia, as well as one of tuberculosis, in which the stress of the disease fell upon the digestive apparatus. In each of these a half-glass of hot water always gave relief when every other remedy failed. The most impressive and permanent results of this remedy seem to be in cholera infantum—hot water being retained when everything else was rejected; and it would so compose the stomach that food could be given almost immediately afterward.

The nauseant medicines mentioned in class 3 are often retained if given in hot water as a vehicle. When an enormous quantity of whiskey has been drunk, and the stomach will not tolerate anything else, hot water will be retained, and then food can be given.

Hot water is less satisfactory in vomiting of pregnancy, yet it is of considerable value in many cases.

In the various manifestations of indigestion, classed 6, 7 and 8—hot water is almost invariably followed by good results. In dyspepsia it may be given before each meal, as well as at other times, to cause the discharge of any undue amount of gas in the stomach by eructation. In this way it affords relief to young infants suffering colic, and it is rarely necessary to prescribe anything else. It has been used successfully in a case of severe palpitation of the heart from dyspepsia.

The decongestive and haemostatic action of hot water have been variously accounted for by gynecologists. Dr. Pitcher, of Detroit, thought that when applied to a bleeding vessel, the immediate effect is dilatation, which sufficiently slows the current to form a clot; and constriction occurring, afterward, the clot is firmly held and the lumen of the vessel closed.

Dr. Emmet says that the direct result is relaxation of the coat and vascular turgesence; afterward, if continued, reaction follows and contraction occurs.

Carl Ritcher, of Berlin, thinks "the contact of the hot water with the partially denuded inner wall of the uterus causes a slight inflammatory irritation, an œdematous transudation, and a swelling of the tissues, principally the submucous, intermuscular, and perivascular connective tissue, by which the blood vessels become compressed and their lumina thereby occluded."

The action of hot water upon the uterine or gastric mucous membrane or upon abnormally full or bleeding vessels in any part of body, may be readily and simply explained by a well-known physiological principle, viz; That of watching a frogs's foot while a needle is drawn across without injuring the membrane. The vessels will presently contract and close, and after remaining so for a few minutes, will dilate to respond no

more, or only partially, to such stimulus. With a stronger stimulus, as that of gentle heat, they will again contract, and such contraction may be lost a day or two.

Wharton Jones found that cold causes speedy constriction, quickly followed by dilatation.

Beaumont, in his observations on St. Martin, found that the ingestion of cold waters was followed by blanching of the gastric mucous membrane, quickly followed by more than normal redness. Now, from whatever cause nausea and vomiting may arise—from the direct contact of an irritant, or the effect of an emetic, or from reflex nervous influences, it is certain in many instances, and probable in all others, that the vaso-motor centers controlling the gastric blood supply are also influenced and gastric hyperemia produced; and this condition being the link in the casual chain which is broken by the contact of the hot water on the gastric lining, the effect fails to follow. In flatulent distension of the stomach the muscular coat, impeded by the gaseous pressure, is excited to extraordinary work, and the gas is expelled. Patients who begin taking hot water to allay nausea, can not only take large quantities without inconvenience, but get to liking it; and at times, when little or no water can be taken, by drinking it hot, enough can be retained to fully meet the requirements of the organism. This fact has an important bearing in therapeutics. Often in both acute and chronic diseases, the issue depends solely on the amount of work that the kidneys will do. In many diseases, the structure of these organs, though not primarily affected by the morbid process, is liable to damage secondarily. The injury may be due rather to the concentration of the urine—the small amount of water—than to the absolute amount of solids; so that the kidneys become clogged with destructive, effete matter if sufficient water fails to flow through them. But dilution of the urine is not the only good resulting from the free drinking of water. The skin is put to work and carries off a large portion of the effete matter that would otherwise have to pass through the kidneys.—*Dr. Douglas Morton, in Louisville Medical News.*

The Bohemian Lying-In Hospital.—The annual report of this renowned institution, drawn up by Prof. v. Weber, and published in the *Allg. Wiener Med. Zeitung*, is a most exhaustive one. In this respect it might serve as a model for similar institutions at home; in another respect—viz., as regards success—the story it tells is as encourging as any can well be. During the year 1882 the confinements in the hospital numbered 2,963. Out of this enormous number of deliveries there were but 7 deaths—0.24 per cent., or 1 in every 423.3 cases. This is what Prof. v. Weber claims as the correct mortality list; but on considering the different items individually it will be seen that, according to the views of some, the figures require a slight adjustment. Thus, 12 women are

reported to have died in the hospital, and 7 others died in the Krankenhaus to which they had been transferred, so that, instead of 7 there were 19 deaths; and, as other lying-in hospitals debit themselves with all deaths occurring in the institution, there is no reason why this should be an exception. The figures given here show how difficult it is to arrive at accurate results either in hospital or out-door practice. They are capable of three different readings—1st. The reading of Prof. v. Weber, according to which all deaths are excluded not directly traceable to child-bearing; 2d. That of including the deaths of all patients dying *within the lying in hospital* within a certain period—say, thirty days; 3d. That in which are included *all deaths* occurring within the above-named period of thirty days. The latter is the mode of reckoning that ought to be adopted, and, according to it, the deaths would be 0.64 per cent., or 1 in 156. According to the mode of reckoning that seems to be in vogue with us, the number of deaths would be put down as 12—0.4 per cent., or 1 in 247. Whatever mode of reckoning be adopted, the figures are very satisfactory, and an immense advance on those of the ten years 1865 to 1874, during which the average mortality was 6.67 per cent. The writer claims that the average mortality during the last four years—1879 to 1882—has been 0.32 per cent.—*Medical Press.*

II.—GYNECIC.

The Increasing Fatality of Cancer.—The last published report of the registrar-general refers to the increased and steadily increasing death-rate from cancer in England and Wales, but expresses a doubt how far the increase may be apparent only, and due to improved diagnosis, and how far to a real augmentation. Unhappily, the consolatory doubt thus suggested cannot be long entertained by anyone who inquires closely into the question. A strict examination of the facts and figures bearing upon it, must lead to the painful and disquieting conviction that cancerous disease is, year by year, becoming more fatal in this country, and that no theory, of more skilful recognition and precise classification than formerly obtained, will adequately account for its increased prevalence. It is indeed somewhat surprising to find the registrar general again propounding the view that the increase of cancer may possibly be apparent only, seeing that this explanation was disposed of finally in the report for 1876, by that accomplished vital statistician, Dr. William Farr: "The increase of cancer," he said, "a strange new growth, is much more rapid than incraase of population. * * As the diagnosis of the disease, when affecting the uterus and internal organs, has been facilitated, by the speculum and other means, the increase may be partly nominal. Diseases now referred to cancer may have been formerly undistinguished, and referred to other heads. But, after every allowance has been made,

it is evident that there is a real increase of deaths by cancer in England." The justice of this observation becomes at once apparent when the returns are looked at. In the year 1847, 4,586 deaths were referred to cancer; in 1879, 14,679 were referred to it. The mortality rose progressively in the five quinquennial periods between 1850 and 1875, from 302 per 1,000,000 living in the first, to 443 per 1,000,000 living in the last. In 1875, the rate was 480 per 1,000,000 living. No one knowing anything of the history of medical practice, will believe that anything like the increase here indicated in the mortality from cancer, which is a disease slow in its progress, invariably fatal in its issue, and marked in a vast majority of cases by symptoms of the most unmistakable description, is to be accounted for by improved methods of investigation, and more accurate nomenclature.

No doubt some cases, which would formerly have been vaguely returned as tumors, are now recognized as cancer in some of its forms; but, on the other hand, probably at least an equal number of cases, which would formerly have been roughly set down to cancer, are now distinguished from it, and ascribed to maladies which have headings of their own; and the facts bearing upon the increase of cancer as a whole, become all the more striking when it is remembered that the mortality from one of its varieties has undergone a marked diminution of recent years. Chimney-sweep's cancer, which was at one time very frequent, is now rarely seen. It has been practically suppressed by Act of Parliament, and affords an instructive illustration of the influence which wisely devised laws may exert in arresting the spread of disease, and even in stamping it out. But, notwithstanding the suppression of chimney-sweep's cancer, malignant disease still gains ground among us.

The fact is, that cancer is growing more common; and that fact should be faced boldly, and should have all possible publicity given to it. To know our danger, is to insure the adoption of such protective measures as are available, and to set ingenuity to work to find ways of escape. To shut our eyes to it is to give it the advantage over us. The alarmist is an objectionable character, but there is a still more pernicious social nuisance, and that is the flattering unctionist, if we may coin from Shakespeare such a name for the being who lubricates everything with the oil of gladness, and polishes each plague-spot on the body-corporate, till it shines so that we cannot distinguish it from healthy tissue. To the flattering unctionist, whatever is is right, progress is universal, and everything *coleur de rose* to souls and eyes that have been properly annointed. Facts that create foreboding may be refined away, statistics that awaken anxiety may be so manipulated as to prove exactly the contrary of their obvious meaning. We have only to stereotype a smile, put our hands in our pockets, and glide into a blissful futurity by the usual route, "straight down the crooked lane and right round the square. And

it is the flattering unctionist who, whenever we are startled by the inroads of disease, steps forward to assure us, with bland confidence, that we are sadly deceived, that the increase is apparent and not real, and that there is not the slightest necessity for taking our hands out of our pockets; and in doing this, he works incalculable mischief. The ignorant public are ever but too ready to listen to any flattering tale, and they are soothed by such assurances, and spared that just appreciation of danger which is the necessary condition of investigation of its source, and protection against it.

With reference to cancer, it seems certain that the public has not yet been awakened to a realization of the truth that it is spreading and claiming a greater number of victims each succeeding year. And surely it is desirable that this truth should be brought home to them; for, once they have grasped it, they will not only sanction, but insist on, searching investigations into the nature and origin of the visitation. The sophistries and sentimentalities of antivivisectionists will be brushed aside, in view of the menacing advance of this dread and excruciating disease; and pathologists will be exhorted to pursue their researches untrammelled, and to sacrifice whatever number of animals may be necessary in the scientific pursuit of means by which its attacks may be warded off, or its ravages arrested. Nor would such researches be undertaken by pathologists without good hope of obtaining valuable results. The tendency of medical and surgical discovery of late has been to remove the barriers which were supposed at one time to separate ordinary inflammation from malignant new growth. It is even taught that inflammatory processes pass by insensible gradations into those of malignancy, while it is pointed out that some of the local applications which are beneficial in inflammation have also an unquestionable influence when used in cancer, in repressing cell-growth, and in retarding its development. There can be nothing, therefore, extravagant in anticipating that we may yet light upon some remedy the internal administration of which will cause a malignant ulcer to heal, or a malignant growth to melt away, just as syphilitic formations vanish under mercury and iodide of potassium.

But, besides the advantageous effect which it might have in freeing and promoting research, there are other ways in which the dissemination of the knowledge that cancer is increasing may be of service. When public attention is strongly called to the disease, its ordinary modes of incursion will be more generally understood than they are at present, and means of prevention or timely treatment will be more widely adopted; for even to-day, when the experimental investigation of cancer has yet to be undertaken, and when medicine is powerless but to alleviate its pangs, the surgeon's knife can do much to ward it off, and to extirpate it. Local irritation is one of the factors in the production of cancer, and this has very often its seat in moles and other forms of congenital excess, as

well as in innocent new growths that are not congenital, in scars and cicatrices, in syphilitic ulceration, sclerosis, and papilloma—in precancerous structures and conditions, in short, that permit of removal.

"It is the conviction which grows with each year's experience" said Mr. Jonathan Hutchinson, in that able philosophical paper on the Local Origin of Malignant Growths, which he read at the Worcester meeting of the British Medical Association, "that, in the rules of practice which would spring out of the full and hearty adoption of the doctrine of the local origin of cancer, rests our only hope of being able to save those who consult us from the horrors of this dreadful malady. 'Too late! Too late!' is written in legible characters upon three-fourths of cancer cases when they come under the notice of operating surgeons. When the doctrine of the precancerous stage shall be widely adopted, and when surgeons generally shall recognize the propriety—let me say the duty—of operation for purposes of prevention, then, and I believe not till then, shall we witness a considerable reduction in the mortality from cancer."

In speculating as to the meaning of the increase of cancer—and nothing but speculation is yet possible—we are led to consider its causes, which may be summed up as senility of tissue, local irritation, and inheritance; and under each of these perhaps some part of explanation of the increase of cancer is to be found. Senility, either of the organism as a whole, or of the particular structure affected, may be regarded as a necessary condition of cancer. The young are almost exempt from it. Of the deaths attributed to cancer in 1879, 75 per cent. were of persons above forty-five years of age, and only 25 per cent. of persons under that age. But it is not senility in its extreme degree that is most productive of cancer, but rather senility in its early stages, when organs and tissues that have been actively engaged in function are just passing into a state of comparative or complete disuse, and when the reproductive energies are subsiding. The forces that have been engaged in building up normal organisations are then diverted towards the formation of heterologous structures; and perhaps it might be argued that premature senility is increasing amongst us, and that the wear and tear of modern life produces an enfeeblement which is favorable to cancerous growth.

It is a lamentable truth, as pointed out by Mr. Leonard Courtney in a recent address at Liskeard, that life becomes more and more toilsome to the masses with the advance of civilization. Mills and machinery, railways and telegraphs, have not really lightened our burdens. They have conduced to the propagation of swarms of human beings, but it cannot be maintained that they have made human existence more tranquil and easy, and less exhausting, than it used to be. The prolongation of life which is alleged to have take place, may be a prolongation of old age, rather than of the years of strength and vigour; and it may be that we

are becoming prematurely old, and are inducing by our modes of life, more frequently than our forefathers did, that state of the living tissues which makes them liable to cancer. The lower animals in a wild state do not suffer from cancer; but when they are brought into domestication, and so have the gift of old age conferred on them by being protected from their natural enemies, they become like men, only in a less degree, subject to this scourge. And some of the habits of civilization, as well as its general evolutional effects, may contribute to the propagation of cancer. Thus it can scarcely be doubted that the enormous extension of the habit of smoking in the last half century must have multiplied greatly those cases of epithelioma of the lip, which often have their starting point in the irritation caused by a pipe. Then, again, our negligence in dealing effectually with other diseases may also favor its dissemination, for if a syphilitic inflammation may become cancerous, it is clear that the spread of syphilis has scattered throughout the population innumerable possible seeds of cancerous degeneration. And each case of cancer, however produced, may, through the transmission of constitutional tendencies to offspring, become the parent of many. Sir James Paget has referred to the case of a person who died of cancer of the stomach, and where two daughters and many of their children (to the number of twelve or fourteen in all) also died of cancer in different organs. It is as yet undecided whether cancer is communicable in any other way except by hereditary transmission.—*British Medical Journal.*

The State of the Uterine Mucous Membrane During Menstruation.—Besides Wyder ("*Ztschr. für Geburtsh. u. Gynäk.*," ix, 1), the names of Kundrat, Engelmann, Williams, Leopold, Möricke, and de Sinéty may be mentioned among those who have made investigations upon this subject. All of them agree that the uterus is deprived of its epithelium during menstruation, and is converted into a wound-like surface. The disagreement is as to the extent of the exfoliation. Williams concludes that there is a complete loss of the mucous membrane of the uterine body, and a laying-bare of the muscular tissue. Kundrat, Engelmann, Leopold and the author are of the opinion that only the superficial layers of the mucous membrane are shed. The four authors first mentioned believe in a fatty degeneration of the menstrual mucous membrane, all considering it as the primary and principal factor in producing the hæmorrhage, while Leopold on the other hand, considers it (the fatty degeneration) as secondary to and dependent upon the hemorrhage, the latter being caused by excessive dilatation of the capillaries of the uterine mucous membrane, with relative insufficiency of the veins. Regeneration takes place, according to the author, from the layers which have not been shed, and which are in a condition of cell hyperplasia. Neither Möricke nor de Sinéty admits that there is loss of mucous membrane

during menstruation, or fatty degeneration, to a very great degree, the process being largely limited to congestion and diapedesis. The author proceeds to the analysis of eight cases, with special reference:

1. To the condition of the superficial and glandular (uterine) epithelium.

2. To the kind of hæmorrhage which occurs during menstruation and the method of its occurrence.

3. To the condition of the mucous tissue, particularly the interglandular substance. He accords to Möricke the credit of having established the following points:

1. That during menstruation a portion of the mucous membrane of the body of the uterus is undisturbed.

2. That the teaching of Kundrat, Engelmann, and Williams concerning a primary fatty degeneration of the uterine mucous membrane is no longer tenable.

3. That the superficial and middle layers of this membrane do not suffer conversion into a decidua. His own views are summarized as follows:

1. During menstruation a portion of the superficial layer of the mucous membrane disappears, the rest remaining. This shedding process varies in extent in different cases, now invoking the entire superficial layer (Leopold, Wyder), again taking only a small portion of it (Spiegelberg). The elements which are thrown off are partly detritus and partly undecomposed matter. In some cases, as in dysmenorrhœa membranacea, small mucous shreds are found, which, however, are too small to cause dysmenorrhœa.

2. This process of shedding is caused by the hæmorrhage of menstruation, and not by primary fatty degeneration.

3. The superficial and middle layers of the mucous membrane, which are left behind, show an abundance of small cells, but no similarity to the decidual membrane of pregnancy. The deepest layers show a cell-hyperplasia of the interglandular tissue, the evident mission of which is to furnish for the tissue which is lost during menstruation.

4. The degeneration of the superficial epithelium is participated in, as well by the glands as by the surrounding membrane in which they are embedded.—*New York Medical Journal.*

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Oöphorectomy.—Tauffer (*"Ztsch. f. Geburtsh. u. Gynäk.,"* ix, 1), says that since Hegar and Battey proposed this operation it has been done more than three hundred times. He dissents from Battey's statements that an artificial climacteric is to be obtained, and that this is the means of cure. He considers that an anatomical change is the end accomplished—a diseased organ being removed, which is of first importance—the climacteric not being the end to be sought for in all cases in which

the operation is required; for example, in a case of hopelessly prolapsed ovary. Hegar's formula is accepted by him—that castration is indicated in anomalous and diseased conditions which are immediately dangerous to life or promise only a tedious progressive sickness, obstructive alike to enjoyment and to occupation. This presupposes that instrumentalities of a milder nature have been used, or can only be used in vain, while the removal of the ovaries will do away with the trouble. Twelve patients have been operated upon by the author without a single death. These cases are grouped upon an anatomical basis, as proposed by Hegar, the groups including the following conditions:

1. A small parovarian cyst.

2. Cystic, follicular degeneration of the ovaries, with degeneration of the stroma.

3. Such a condition of the uterus as rendered menstruation impossible or very difficult, the ovaries, meantime, proving to be normal.

4. Structural disease of the uterus; e.g., myo-fibroma.

5. Pathological displacements of the uterus and ovaries.

6. Circum-uterine and circum-ovarian inflammation, inflammation of the pelvic connective tissue, and salpingitis.

7. Mental diseases connected with the family history. The following is the *résumé* of the cases: ten double, two single operations; no deaths; continuance of inflammation in tubes in two cases; immediate menopause; in four; menopause after an apparently normal menstruation in one; metrorrhagia continued in two patients who had been operated upon for fibroma uteri. In three the periods continued normally for a number of months (while the patients were under observation), caused by inflammation of the tubes or of some other portion of the uterus or its attachments. The author sums up his experience in the following propositions:

1. With proper precautions, castration is not attended with great danger; the average mortality is now less than ten per cent.

2. The operation should be done antiseptically, with the spray; the abdominal cavity should be closed; drainage need be employed only exceptionally.

3. The limitation of the operation to those who are not near the climacteric is not sound, for the period at which the climacteric occurs is variable.

4. The precept of Hegar's—to be able to palpate the ovaries, if they require removal—is impracticable.

5. It is usually best, for obvious reasons, to remove both ovaries, especially if one shows disease.

6. Occasionally the removal of the tubes is necessary, especially if they show the slightest disease.

7. Hystero-epilepsy is curable by castration.

8. Hysteria is often traceable to ovarian disease.

9. The question as to the influence of ligation of certain large vessels of supply in uterine fibromata, without castration, is well worthy of consideration.

10. As to prognosis concerning the climacteric, all inflammation in organs adjoining the ovaries will have a retarding influence.

11. The final result of the operation often requires months for its determination.

12. The question as to how far diseases of the female sexual organs are influential in the development of certain psychoses is an open one.

13. The same is true of the question as to whether psychoses are curable by castration.

14. In the interest of the profession, some such grouping of cases should be followed as Hegar's, thus permitting a rapid accumulation of facts necessary to one who is studying the clinical history of the subject.—*New York Medical Journal.*

III.—DISEASES OF CHILDREN.

Bimanual Detection, and Removal of Stone in Children.—A case which well illustrates the bimanual method of calculating the size and situation of stone in the bladder, has recently been under the care or Mr. Churchill at the Victoria Hospital for Children.

A boy aged 9 years and 10 months, very thin and spare, and having a conjugate diameter of only 2¼ inches, and the distance between the two tubera ischii 1¾ inches, was put under the influence of a mixture, containing alcohol, one part; chloroform, two parts; ether three parts.

Suspecting that he would have to deal with a large calculus, Mr. Churchill, before sounding the patient, passed the left index finger into the rectum, and illustrated the method of mapping out the contour of a calculus by digital examination.

The calculus, being poised upon the palmar surface of the index finger in the rectum, was, by a gentle concussion process, brought within range of the thumb and finger of the right hand in the suprapubic region.

Having satisfied himself as to the relative size and weight of the calculus, Mr. Churchill proceeded to remove the stone by the usual lateral lithotomy operation. Finding some difficulty in extraction with the forceps, and also with the scoop, although both the external wound and that through the neck of the bladder were enlarged sufficiently to accommodate the stone, Mr. Churchill resolved to abandon the use of instruments, and extract the stone by manipulation. The left index finger was introduced into the bladder, and the calculus which was a conglomerate mulberry one, almost spherical (one inch and one-tenth in diameter, weight 280 grains apoth.), was hooked by the finger straight from the fundus to the neck of the bladder.

Mr. Churchill then introduced the right index finger into the rectum, and, by cautiously removing the left finger, which fixed the foreign body, the calculus was tilted up, and pressed through the opening in the bladder, and then through the external incision.

Mr. Churchill showed, by grasping the stone in the forceps, that it would have been necessary to enlarge the incision to nearly twice the size, to extract it, without laceration, in the grip of the forceps. Considering the limited space available for incision, this bimanual method of extraction was undoubtedly the safest and best. Ten days after the operation, the patient was doing remarkably well.

Another peculiarity of the case was the extreme dilatation of pupils, as though the patient were under the influence of atropine, but due apparently to anæmia.—*British Medical Journal.*

The Use of Quinine in the Diseases of Children.—M. Jules Simon, of Paris (*Revue des maladies de l'enfance*, February, 1883), calls attention to the necessity of bearing in mind the curious and unexpected forms in which malarial poison manifests itself in children no less than in adults. The obscurity of the malarial origin is often greater in cases affecting children than in those affecting the mature. Not only may there be no fever, but there may be no periodical exacerbations of the disease. There may be no reason to suspect the true nature of the attacks, except the history of exposure in a malarial region. Nevertheless, if the affection be due to malarial poison, all treatment that does not take the cause of the disease into consideration will signally fail, while quinine will effect the most astonishing cures. M. Simon cites four cases of children suffering from irregular forms of malaria, some of which had resisted all other treatment, all of which, however, yielded quickly to the sulphate of quinine.—*N. Y. Medical Journal.*

Diarrhœa in Infants.—Dr. Jules Simon, among other forms of diarrhœ, alludes to the cerebral, and says: "Diarrhœa may assume a cerebral form, that is, it may be accompanied by certain cerebral phenomena or by eclamptic convulsions, even although the diarrhœa may be but slight. As a result of a profuse drain, a comatose condition may be developed. Again, meningeal symptoms may develop and meningitis be simulated." Many of the reported cases of meningitis with recovery are, the writer thinks, only cases with meningitic symptoms. The various forms of diarrhæa are discussed in detail. For the treatment of cholera infantum, the writer recommends (1) preventive measures to be adopted during the premonitory catarrhal stage, and (2) after the full development of the affection, alcohol internally and externally, and mustard baths.—*Edinburgh Med. Journal.—Medical Progress.*